THE LANCET.

No. 179.] LONDON, SATURDAY, FEBRUARY 3. [1826-7.

MR. ABERNETHY'S

PHYSIOLOGICAL, PATHOLOGICAL, AND SURGICAL OBSERVATIONS;

DELIVERED IN THE

ANATOMICAL COURSE OF LECTURES,

At St. Bartholomew's Hospital.

On the Intestines, or Alimentary Canal.

Small Intestines. With respect to the physiology of the small intestines, the digested aliment passes forward to the *jejunum* in a continued track of small intestines, and at that time the bile and pancreatic liquor have been observed to flow copiously into the alimentary canal; and indeed it is natural to suppose that the change the food undergoes in the small intestines must be owing to a commixture of those liquors. And so it may be, but we have no proof of it; and what I have got to say about bile or pancreatic liquor cannot come in now; it must come in with the physiology relative to the liver and pancreas. Relative to this, Mr. Hunter observed that the bile did not seem to commix with the digested aliment; he said it seemed to flow over it, and that the bile and excrementitious part went into the lower part of the bowels, and moved on. Now Sir Astley Cooper, in those lectures he delivered before the College of Surgeons, and for which he made numerous experiments, gave me additional information as to this, which, if it be correct, I hold to be very valuable: He says that the digested aliment coming on into the intestines, coagulates to the very surface of the tube, adhering to it most firmly and most tenaciously. This fact he demonstrated, for he produced a piece of the intestines with the chyme, as I may call it, adhering to it; so that when the intestine was slit up, you could not see the villous coat; with a pair of forceps he pulled off a thick slip of chyme and sheath, and beneath it the villous coats appeared, and were injected as red as possible. He said the digested aliment acquired a kind of coagulation or adhesive quality, by which it firmly adhered to the internal coat of the small intestines. Now you know the power which might be called spontaneous coagulation seems to be a very peculiar and a very universal property in the nutritive fluids of animals. It is extremely unlike what we see happen in fluids, not of a vital nature; nor is this like a chemical change. Mr. Hunter believed that this was the result of vital properties; that the blood—the coagulation of the blood, was the result of vital properties; for blood sometimes coagulates and sometimes does not, apparently under the same circumstances. Then Sir Astley Cooper was led to believe that the digested aliment had that peculiar property of self-coagulation, as it were, which would enable it to cling to the surface of the intestines, as coagulated blood clings to the side of an artery in an aneurism. It seemed to him to show that the stomach had not only dissolved the food into something that had a very peculiar nature, but had conferred on it a power of self-coagulation which enabled it closely to adhere to the surface with which it was in contact.

Now I say, if this is true, and I tell you there was a demonstration of the truth of it to a certain extent, it seemed to throw new light upon the subject to my mind; it showed how the juices of the intestines were not wasted, for what was secreted from the surface of the intestines would be immediately applied to this chyme, and act upon it, and convert it into chyle. Whatever properties bile and pancreatic liquor may be supposed to have, analogy, which is our guide and rule in all circumstances as to the formation of opinion, would lead us to suppose that the change the food undergoes very much depends on the juices that commix with it. The surface of the intestines is excessively vascular, and it has a villous origin. We must believe it is an origin of great secretion; we have evident proof of its being so in morbid affections; the quantity of morbid secretion poured out is very great indeed;

but of the nature of the healthy secretion of *succus intestinalis* we are as ignorant, as we are of that of *succus gastricus*. And what Sir Astley Cooper says is satisfactory to the mind, inasmuch as it shows how those juices are not wasted; why their action is limited to that which is converted into something ulterior for the benefit of the body.

Now what we know is this: that as soon as this stuff is in the lacteals, it resembles the blood,—it is chyle; open a lacteal, catch the contents, and what do you find? You find a fluid, which, like the blood, separates into serum and crassamentum; there's concretion, there's a crassamentum formed, analogous in every respect to the crassamentum of blood. And what is more curious still, this liquid abounds with globulous particles, like blood; they are not red, to be sure; in that only does it differ from blood. So that you see how speedily the change of digested matter takes place, which is designed to be effected. What is the object of digestion? The conversion of the food into something resembling the substance of body, and for the support of the body.

Now that 's all I have to say concerning chylification; and I go on. The residue of the food and the bile, which is certainly incorporated with it, is urged on by the *peristaltic* motion of the intestines, and eventually propelled from the *ileum* into the *cæcum*, the *caput coli* or *cæcum*. Well, now I have to speak of the structure of the large intestines. Mr. Abernethy described the structure, and then proceeded:

With respect to the *valve* to be found here, it prevents the fœtid air from getting upwards; but does it never get upwards? O yes, often, often, often, when the valve is in a *morbid* state. Where the valve does not perform any function attributed to life, how often do we hear it reported that people have vomited up clysters; but is it the clyster, or is it the contents of the stomach, scented only by the clyster, that is vomited? I believe the latter. But don't understand me to say that people do not vomit up clysters, because I have seen cases in which they have, and I have known the people to live long and happily afterwards. I remember a lady who had a state of bowels which caused every thing to go the wrong way; no medicine could bring back a proper state of the bowels, no stools could be procured; she could take nothing, she had had nothing for a week; a copious clyster was ordered, of water-gruel and oil; she vomited it; the oil was seen floating on the top of what she did vomit; how could the oil have come there, had it not been for the clyster? And yet, I say, she recovered, and lived long and happily afterward.

In cases of *hernia* too, there is a stinking matter brought upwards, and you smell the most unpleasant breath of the patient. O it's a very horrible sign; the chances are, that that person will never live, I am sure.

Functions.—Now as to the functions: undoubtedly there is a great change wrought in the alimentary matter in the large intestines, a great and a sudden change. You may slit open the alimentary canal, as far down as the ileum, and you may find stuff in it, but it has not at all the odour of *fæces*; but cut a single line further, cut but a line of the cœcum, and the nose is assaulted by a tremendous smell. Formerly, people thought the putrid smell of the fæces was a chemical change on the food; but it so suddenly takes place, that this opinion is quite untenable And to what can we impute this change? Why, analogy leads us to suppose, that it is a change which takes place of the food, by the quality of that which it secreted, the succus intestinalis of the large intestines. And what is the nature of the change? The fæces have a bad smell, but are they putrid? O no, not at all. In health there is no putrefaction in the large intestines; but the contents of the large intestines suddenly go into putrefaction, and the product of it is *ammonia*, as you may believe, if you go into a common necessary, for there the smell of ammonia is perceptible; but is there ammonia in the fæces before they are expelled? So much the contrary, that *acids* are found in the fœcal matter, so that if there was ammonia in them, that could not also be found in them. There is no chemical decomposition in the large intestines when they are in health. Chemical decompositions take place in the stomach; food there becomes rancid. When the succus gastricus does not produce its proper effect, several chemical changes take place in the food. *Hydrogen* is found there; the air that is discharged is inflammable, as many school boys know. (Laughter.) Now all this is owing to the state of the health, and in health there is no chemical decompositions, even in the large intestines, so that whatever notions we may form as to changes in the food, wrought in the large intestines, we may attribute this as one, that the change which is produced in it is protribitive of the food going into those pernicious chemical changes which it would otherwise undergo. Pernicious chemical changes which it would otherwise undergo, I say, because, if you think for a moment, you must admit, that if the alimentary matter was allowed to go on into noxious putrefaction in the large intestines, it would produce most horrible fever. Do we not know, that when putrid matter is absorbed and goes into the vessels, that horrible fever is the result? I mean *putrefaction*. You don't use the word *putrid* in a vague manner; we use it to a thing having a bad smell. You may apply it to the fæces. Fæces have a pecu-

liar smell, but they have not the smell of putrefaction; they are fœtid, we may say, but not putrid.

Well, this may be disputed; but there is reason to believe that there is a change wrought there which still produces something nutritive. For in *herbaceous* animals, the *cæcum* is exceedingly capacious, and there the residue of the food is not likely to go into putrefaction. More particularly in the *horse*, which does not *cook* its food, as I may call it, which does not ruminate, the cœcum is large; and, on a former occasion, I told you it was a sort of reservoir for the liquor the horse drank. Well, then, we have reason to suppose, that the food does undergo a second sort of digestion here, capable of converting that into nutrition which is capable of it. And here again, Sir Astley Cooper affirmed, that whatever was nutritive clung to the surface of the intestines; acquired that property of adhesiveness, of coagulation, which enabled it to cling to the surface of the intestines, while that which was not fit for nutrition was gradually propelled forward by the peristaltic motion of the intestines, and ultimately expelled. O there is a great change takes place. I have always said in these Lectures, that we talk of *chylification*, and why should we not talk of *faecification*? for there is a wonderful change produced in the intestines; and though we do not know the extent to which that change is produced, yet it is a very important change. I have sometimes said, that a very old man, a queer character, an humourist, who lived in the early period of my life, whom I knew, a *Dr. Maunsey*, saw these things very clearly, and who left his body to be examined and dissected after death. He said, that all the surgeons in the kingdom, let them do what they would, they could never make a *t—d*. (Roars of laughter.)

Morbid Anatomy.—Now I have done with the functions of the alimentary organs, and next I have to speak of the morbid anatomy; and I always consider this part proper to be begun with requesting gentlemen to make up their minds as to what they are to do when people have taken poison. You may read books, and you may use the stomach pumps, on which I have but little to say just now; but I know it is most important that every man should have his mind prepared for what he is to do on such an occasion, for then he can have no time for consideration. You may make them drink, you may make them vomit, you may thrust your finger down their throats, and you may extract with the pump, but the object is to neutralize as much as possible the noxious quality of that which has been swallowed, and to get them to throw it up. There are even *salts* which admit of dissolution, and the object is to get that out of the stomach, which has been taken, by inducing vomiting, or by the use of the pump. Now I have this opinion upon this morbid anatomy, that it does not carry with it the interest to my mind that it does to many others, because the appearances are so excessively various; and yet, as far as general classification goes, O, we see the same sort of diseased actions fill up the same sort of diseased structures in every part of the body.

The only way in which I could explain what I know relative to the morbid anatomy of the alimentary organs, in an useful manner, is to speak of them in their morbid state; first in their *peritoneal coat*; next in the *internal coat*; and lastly, in their *intermediate coat*, which I may call the walls of the vessels.

Peritoneal parts.—Well, with regard to the peritoneal parts, what have I to say? What have I to say, why, it was a thing generally believed, when I was young, that when a person had *peritonitis*, it would either kill him or he would get the better of it!!! People were not then aware of the *chronic peritonitis*, which would go on for a very long time indeed, till it changed the whole and entire form of the abdomen. I know I have been often called upon in cases of this kind where, if I had been brought into a room and desired to tell what I saw, I should never have known that it was the cavity of the abdomen. The *viscera* are all united together; sometimes there is no cavity at all; sometimes in trying to separate the viscera I have torn the bowels asunder without separating them. The peritoneum every where rough and thick; parts adhering, and the vacancies between those parts filled with fluid and tubercles; the peritoneum studded with tubercles.—Here are specimens of that kind. I therefore was exceedingly pleased when I saw published an account of the nature of this persevering disease, which gradually destroyed life.

Internal Coat.—Well, now, as to the internal coat; why it's liable to ulceration, and that's a very odd thing. How ulcers form there is curious. Here are specimens of ulcers in the stomach, and the stomach is liable to have many ulcers in it, but those ulcers will of course bleed occasionally, and yet they may eventually heal. Now I say this is curious, that an ulcer should penetrate, beginning in the internal coat, through all the coats of the stomach, and yet leave no aperture out of which the contents should escape. I say this is curious; it happens both in the stomach and in the intestines, and I have seen it frequently. Curious at first sight only, not curious upon reflection, because as the ulcer proceeds to the external coats, it produces inflammation in the peri-

toneal coat, so that this peritoneal coat adheres to the contiguous bowels. The stomach may adhere to the walls of the abdomen, and the aperture is shut up by this adhesion. Dr. Bailey has given, in his plates of anatomy, instances of the stomach in this state, and I had instances of them too, many such instances, among my preparations, but they go—they vanish! With respect to ulcerations, of all the coats most likely to be attacked, Dr. Bailey says, it generally begins in the mucous structures, all through the small intestines, as in this instance, in which the preparation was taken from a man who died of *diabetes*; and when I have to speak of diabetes, I shall declare it as my opinion, that the chief cause of that state of urine, is an imperfection in the digestive organs; that the vegetable part of the food is not digested, and that it is imbibed into the blood undigested.

Well, now, in the next place, as to ulcerations in the small intestines, I know of nothing more that is to be said about them. It is the same with respect to the large intestines; there are ulcerations also to be found in them.

Intervening parts.—Now then, in the intervening parts of the coat, the walls of the vessels, O it is very curious to observe what is set up there; that the walls become excessively thick, the internal coat, and the peritoneal coat not appearing to be diseased. Now here is a stomach, which you will hardly recognise to be a stomach, but the *form* shows that it is one; it is great at one end and becomes less at the other. This was given to me by a gentleman who was the surgeon of the patient; it was the stomach of a poor watchman, who went round calling the hour as long as he could, when he could do it no longer he went into a poor-house; he was always complaining, and the surgeon who attended him thought there must be something very curious in his internal composition, and upon examining him this was found to be the state of the stomach. So with respect to the intestines lower down, they present the same appearances; parts of them are excessively thickened, whilst other coats of the same portion remain undiseased.

Well, the *scirrhous* disease begins in this manner; it is found intermediate between the two coats, but it is of that nature that eventually ulcerates. Dr. Bailey has given the best description of incipient scirrhous, in the case of *carcinoma*, that can be given; and this disease is found in the *pilorus*; it is found in thick, white bags.

Strictures.—Now here are specimens of stricture in the *rectum* and other parts, not of the cancerous nature. Though some *strictures* lead to carcinoma, yet all strictures are not of a cancerous nature, and it's curious to know how frequently strictures occur in the alimentary canal. In all mucous canals, the *urethra*, the *œsophagus*, and in all the *alimentary canals*, stricture occurs; and in these cases there is something wrong in the muscular power. A disposition to contraction becomes established, thickening goes on, and a permanent sort of diminution of the tube is induced. Meeting with these strictures all through the alimentary canal, the consequence in some cases is, an accumulation of the fæces above the part contracted; this is exceedingly complicated. I can tell you of a striking case of this kind. There was a young woman who came out of the country that I once saw, and having a grand disorder of her stomach she was of course advised to consult *Doctor* Abernethy, and she brought a statement of her complaint with her. I told her I was not a physician; that I could not understand her case by the statement of others, but that I would examine her if she pleased; at the same time I advised her to get better advice. She allowed me to examine her; I traced the boundaries of a large swelling in her abdomen. I said to her, you had better stoop and lean forward with your elbows on a table, for by doing that, if there is any swelling in the belly it's sure to descend. I went on examining her, working in my own mind to make out what her complaint was. In the course of examining her I put my hand on the surface of the swelling on the belly, and upon gently pressing it I heard a rumbling and jolting sort of noise; I went on, and pressed it altogether away. It was some contraction of the alimentary canal, and an accumulation of the alimentary matter above it. I requested her to see Dr. Bailey, and I was much gratified to find that he gave her the same opinion, both as to the nature and treatment of what she had complained of; which was, to take care to excite a purgative action of the bowels above, to cause them to carry down, through the contracted part, that which ought to pass. She soon got rid of any tumour in her belly, and I fancy she got well; at least she did so as far as I ever heard any thing of her.

Contractions and *malformations* might be spoken of. It should be remembered that there are malformations occasionally occurring in this organ; a curious sort of process going on in the alimentary canal. This should be remembered; for supposing one of those things to occur in the case of a *hernia*, it might slough off, and do a person no harm, I am sure.

Worms. You find worms in the alimentary canal. There are worms of different kinds to be met with there; one long worm, pointed at either end, which they used to

call *lumbricoides*. Then you have the *tape worm*; and there is generally but one of this kind to be met with, so that the French have called it *ver solitaire*. To be sure it breaks, and a great deal of it may be brought away, but it grows again, and no good is done unless the head of the worm is loosened, and the whole of the worm brought away; then the patient is cured. There are other intestinal worms met with occasionally, but those I have mentioned are the most frequently found.

Involutions. Next of involutions,—that 's a curious thing; but Mr. Hunter published a paper upon this subject, showing how those involutions were likely to take place: *intus-susception* it is called. In these cases the bowels act capriciously and spasmodically. Sometimes this happens to an enormous degree; it has been to that degree that the valve of the ileum has been known to present at the anus. The French memoirs abound with such cases; and when I used to read them, before I met with similar cases, I used to say,—Odd hang it! these Frenchmen are always having their bowels come away; but such cases are very frequent here. There was a woman in this Hospital, in whom the *intus-susception* portion perished and came away, and yet she had natural stools afterwards; but she died.

Here, however, I talk of things for which there seems not to be any direct remedy; but still it is right to call your attention to them, and having done that, I have finished for to-day.

FOREIGN DEPARTMENT.

PATHOLOGY.

*On the Predestination of the Sexes, as an Appendix to the Treatise on the Equality of the Numbers in the Sexes.** By HUFELAND.

I THINK that in my Treatise on the equality of the Numbers in the Sexes, (uber die Gleichzahl der Geschlecter,) I have indisputably proved that the numerical proportion of both sexes in the human race (as twenty-one of one sex to twenty of the other) is equally distributed over the earth, and that this does not, and from its nature never can, depend on climate, the planets, or any external or accidental cause. I moreover hope to have proved that the act of conception determines nothing on the subject, but that the difference of the sexes exists preformed in the ovum or the germ of the future individual, to the vivifying of which the semen of man alone contributes. On this I ground the opinion, that the cause of the difference in the sexes must be looked for in a higher region, and one not subject to casual influences. We should no longer speak of the development of the sex during pregnancy, as something then taking place or then determining itself. The organs of generation make their appearance at this period, and for the first time develop themselves; but their basis and internal germ, the sexual difference itself, is given with life, and is connected with the earliest germ of the future existence, even before it is called into life. The original type of the human race is double, male and female, even determined at the creation. This will be the more readily granted when we recollect, that the difference of the sexes does not consist merely in the organs of generation, but in a difference of the whole form, structure, and essence, not only of the corporeal but also of the mental powers. Do we not see from the earliest infancy, long before the development of the proper sexual life, an essential difference in the form, structure, and dispositions of the boy and the maid? And do not anatomical observations perfectly prove, that the primary traces of the sexual organs are those which first show themselves in the fœtus; and recently Mr. Ritchie* has informed us, that even in the ovum this sexual difference is evidently given and determined.

Notwithstanding this problem has been made the subject latterly, in France, of investigation and comparative researches, both as far as regards the human subject as well as several of the classes of animals; and the labours of Ollivier, Prevost, Dumas, and Giron deserve our thanks and acknowledgment:† the question was again propounded,

* Hufeland's Journal der Practischen, Heilkunde, October 1826.

We have inserted the following article for the purpose of showing in what manner a clever man may perplex himself, when speaking on a subject of which he and others are ignorant, rather than for the information it contains. ED. L.

* H. Ritchie found that those eggs in which the air-vesicle lay at the obtuse end of the shell, were male; but when in the middle, female, which could be easily recognised by holding it against the light.

† Annales des Sciences Naturelles, May 1825.

whether the mode of living, locality, time of the year, climate, &c., had no influence; but these circumstances were not found to have any cause in the production of the difference in the sex. The proportion of births of the sexes maintained itself equally under all the varieties of these external circumstances. The influence of age and constitution has been the last subject of investigation, and from numerous carefully-conducted observations on the domestic animals, the following results have been obtained:—

Old male and female, produce the sexes in equal proportion;
Old male and middle aged female, more females;
Middle aged male and young female, more males;
Middle aged male and middle aged female, equal;
Middle aged male and old female, more males;
Young male and young female, equal;
Young male and middle aged female, more females;
Young male and old female, equal.

From this it will be seen that the proportion, by calculation of all these combinations, is equality of the sexes, and that the determinate proportion of nature, 20 to 21, will be the result.

But, granted that these observations are by far not sufficiently numerous and satisfactory to allow of conclusions being drawn from them, still the comparisons are not applicable to the human subject, as will be perceived by the following remarks:—

1. The matrimonial connexion of a middle aged man with a young wife is, from very natural causes, the most frequent. It takes place in the proportion of four to one of the other contracts. There would then, according to the above table, be a greater predominance of the male sex, which is by no ways the case.

2. But the extra-matrimonial connexions must also be taken into account, from which a greater proportion of males would also be the result; but this we do not find.

3. The long wars, where at last all the male youths of a nation are destroyed, as for instance in the latter part of the seven years war in Prussia, the long revolutionary war in France, a very considerable increase of females might have been expected, but it was not the case. The determined numerical proportion of the sexes remains always the same.

4. Matrimonial contracts with persons of the same years should, at all periods, have for the progeny as many males as females; but this is not found to be the case.

6. Experience shows, that marriages of middle aged men with young women frequently take place, from which a female progeny is the result.

6. And if all the above combinations were granted, still the determined and prevailing proportion of the sexes, from 21 to 20, is not to be explained by them. The same applies to the objections about the disproportion between the strength of the males and females, which has also been brought into use to explain the exceptions to the above results.*

M. Prevost has called to his assistance in this difficult point, the old hypothesis about the spermatic animalculæ, and applied it in a new manner. He assumes that these animalculæ (the quickening powers of the male) form the nervous system; the female quickening power, on the contrary, the blood and plastic system; and in this way a disproportion in the sexes may be caused. But in my opinion the strength of the male serves only to call into life the germ of the future individual which belongs to the mother, and to communicate to it different degrees of strength, but in no way to determine the sex. It may be regarded as a settled point, that in the female ova, the germ of the future individual, the sex is determined and preformed. All other explanations rest on combinations, subjected more or less to chance. Such cannot be the case in a point on which the existence of the race depends, and where such wonderful regularity and constancy in the numerical proportion of the sexes is always preserved.

---

* The following are the results of some observations instituted by M. Giron de Buzuringues and M. Morel Vinde, on some animals. These observers conceived that something conclusive might be learned as to the sex of the progeny from the external appearances of the parents. The males, for instance, with a proportionally strong thick head beget more females. Males, on the contrary, with a small head, large belly, and broad pelvis, beget more males. Females with thick, broad heads, however, bring forth more males, and those with smaller heads more females.

MEDICO-CHIRURGICAL REVIEW.

January, 1827.

"Have you seen Johnson's Jan. 1827?" "No; what about it?" "He commences the new year and his quarterly labours with a review of Paris on Diet." "Johnson is incapable of criticising any work composed by that author; but after trying his precepts on his own stomach, I suppose he has had the presumption to publish their effects in the shape of a monograph—is that it?" "Precisely; whenever he assents to, or disagrees with, the doctrines laid down in the book, he invariably appeals to his flatulent entrails as to an infallible oracle to decide the question at issue." "Bah! to sit down over any gentleman's production, and say it is good, or bad, just as the sulphuretted hydrogen puts him in or out of humour; what's the tendency of his ventriloquism on that splendid work?" "Evidently afraid to condemn, he has strung together a series of common-place panegyrics on the occasion; but it is quite apparent that his approbation is not the heartfelt gratitude, the ardent admiration, or sympathetic satisfaction with which one man of science and genius ever beholds the progress of a kindred spirit; for whenever a speculative point or disputable fact affords him an opportunity to cavil, you immediately see the poisonous drivel of jealousy exuding from his pen into a note at the bottom of the page, like the venom-drop at the end of the dying wasp's sting mingling with the sweetness of the liquid which only gives death to the crawling insect." "What else could you expect from the Sisyphus of medical literature,—doomed to the ineffectual toil of endeavouring to roll his leaden Journal up the steep of notoriety, but sinking deeper and deeper into the gulf of obscurity at every effort,—than envy, bitterness, and asperity? In what manner does he perpetrate his malevolence; by extracts, or analysis?" "By the latter; and openly confesses that want of space to contain, and mental powers to compress, only prevent him from transferring the whole treatise into his pages: and this he calls making himself useful." "To himself it may be so, but any such attempt, you know, is barefaced piracy, aggravated by the affectation of honesty. Reviewing, indeed, has become of late a downright nuisance; either the most audacious robberies are in this manner committed on writers, or impostors are encouraged to compile bad books with the sole intention of having them puffed off in one of the journals; for is there a newspaper you look into but you will find some catch-penny publication advertised with a critical *probatum est* appended to its tail, like a slough hanging from an ulcer?—Johnson's analysis, then, if I understand you rightly, bears the same relation to the composition from whence it was manufactured, as a table of chronology does to history." "Much the same; in dissecting off those parts which gave continuity, proportion, and rotundity to the narative, the skeleton even of the work has been broken, battered, and become carious under the pickaxe of the critical pioneer. But notwithstanding this Gothic havoc, the '*disjecta membra poetæ*,' the terse style and research of the man of science and letters occasionally shine out amidst the rubbish of his reviewer, like fragments of gold separated from their parent rock in the alluvial deposit of the mountain torrent." "Yes, in sponging out the more delicate tints of the picture to make it the more transferrable to his lopping-paper columns, he has even destroyed the outlines.—does he add a new thought, or an unknown fact to compensate such a spoliation?" "No; the utmost stretch of his intellect extends no farther than contradiction; but the answer of Blumenbach to the great phrenologist, 'that what is new is not true, and what is true is not new,' is quite applicable to his crude emendations. I will just give you an instance: Dr. Paris, with a view of making himself intelligible, defines dyspepsia in the following words:—'I define indigestion to be a primary disease, in which one or more of the several processes by which food is converted into blood are imperfectly or improperly performed, in consequence either of a functional aberration, or organic lesion.' You will perceive that the introduction of the word 'primary,' in this definition, has involved the writer in a double absurdity, as he subsequently states that this 'primary disease' may be not only a 'consequence' of 'functional aberration,' but also of 'organic lesion.' Just observe with what a treacherous exultation Johnson unsheathes his rapier on the occasion, and instead of running it through his antagonist, commits suicide on himself:—'Dr. Paris has created no small confusion here, by attempting too much. If the definition had terminated with "functional aberration," all would have been well; but by including "organic lesion," it involves a contradiction, and comprises a singular error in pathological reasoning. What! is indigestion defined to be always a primary disease, and yet sometimes a consequence of organic lesion? To say indigestion is a consequence of functional aberration, is intelligible; because such aberration constitutes the disease itself,' &c.". "Excellent critic! here

he commits a more ludicrous blunder than Dr. Paris, for how can indigestion be called primary, when it is consecutive to functional aberration, any more than when the disease is the product of organic lesion? for, being equally a secondary phenomenon in both cases, it cannot, of course, with propriety be defined primary in either; but, above all, how can it be said to be a consequence of, and at the same time be, the functional aberration itself, unless, indeed, we are henceforth to consider the effect as identical with its cause; an absurdity which logic has taught us to look upon as an impossibility. Much on the same principle, Johnson, I suppose, would call symptomatic fever a compound fracture, because the latter preceded, or was coexistent with, the former. I am told he has written a book on the subject himself; what sort is it?" "A compilation of Darwin, Spallanzani, Philip, Paris, Kitchener's peptic precepts, baptized with a new name, and intended for the perusal of the hypochondriacs, who will consult it before every meal until some more fashionable treatise arrests their attention; is it not monstrous that there should be so much noise made about what people eat?" "Men's brains are fast descending into their bellies. Johnson's, you plainly see, have already cleared the diaphragm, and eructated from its Æolian habitation a new system of ventriloquy." "It is a melancholy fact; how can you account for it?" "Readily—by man's disobedience to that all-wise mandate, in which it is said, that 'with the sweat of his brow he shall earn his bread;' or, in other words, idleness and luxury are the cause of this general desolation amongst the stomachs of Great Britain. Humanity, thus condensed in cities, now breathing the exhalations of filthy streets, now the hundred-times respired atmosphere of drawing-rooms, made up of gas, civet, and perspiration; feeding on aliments tortured through all the alchemy of stoves and stew-pans, until the original substance of experiment is no longer recognizable to the taste or touch; exerting merely as much muscular motion as is sufficient to bear it from one scene of luxury to another, feeling no medium sensation of the body, or consciousness of the mind, between the extremes of voluptuous enervation, and the fever of passionate excitement; poisoned, pampered, sluggish humanity, vacillating between the cook and the apothecary, between the snake-whip of Tisiphone, and the oblivious cup of Circe, is it surprising that it should ferment and putrify, that its limbs should be palsied, its eyes sightless, its cheeks blanched, its breath pestilence, its life but an impersonation of death and corruption? If the whole tribe of dyspeptics were put under your care, how would you treat them?" "Condemn the whole race indiscriminately to hard labour and coarse diet—the gentlemen, to field sports—the ladies, to catch butterflies, on bread, bacon, and buttermilk." "Johnson, for instance, how would you set about putting *his* stomach to rights?" "Deprive him at once of pen, ink, and paper—place him astride a tall crooked-legged Scotch mule, without a saddle—trot him forty miles a-day over moor, mountain, and morass, in the ruggedest parts of the Highlands, and whenever his cockney heart shrunk within him at the prospect of a precipice, take him down gently—tie him to the beast's tail—crack the whip behind—whirl him over the gulf that yawned a thousand feet beneath him—and if, after a few months' discipline of this kind, his duodenum growled, or his mind was disturbed by this nightmare, holy writ is no revelation." "Would it be safe to entrust him with you in the solitude of the hills, where the chasm gaped, the rocks frowned, the eagle and falcon screamed for their prey, and nature wore an aspect of murder?" "Perfectly. Johnson is an object of compassion, rather than of resentment. Destitute of the sagacity to perceive the changes which were taking place around him, events took his vigilance by surprise, and there he stands, fretted, chagrined, at war with the whole world, and bellowing after the "march of intellect" like a traveller who outslept the hour of starting bawling after a mail coach, to the great amusement of the public and passengers. What other works does he grub the marrow out of, besides that of Dr. Paris?" "The Edinburgh Medico-Chirurgical Transactions are strewn over one half of his pages in such a mangled state, that the proprietors would be at a loss to know their property, if the titles had not been preserved." "The pirate! what must the poor chaps think, on seeing their brains scattered about as rudely as if they had been discussed by the tomahawk of a back-woodsman? How does he fill up the periscope?" "With scraps of hospital reports, and of lectures filched from the pages of THE LANCET." "Johnson, you see, has evidently a face to do anything in this way. Do you recollect his abuse of that publication for reporting lectures and hospital practice? and now the man of brass has the assurance to transcribe the labours of a Journal into his pages, which had been the object of his invariable detraction! His next transformation will be into that of a radical reformer. Does he speak of medical politics?" "As witty, wordy, and classical as ever on the subject of reform." "There is really some danger of his dying one day or another of making a joke. In every one which he attempts you see the marks of laborious parturition,

like the effects of strangulation on a still-born fœtus; and his classical quotations (did you ever observe them?) are all carefully selected from that compilation of the beauties of the Latin Poets to be found in the rules of Syntax. Has he a deliberate article on the subject?" "His opinions on that point are principally drawled out into a commentary on Mr. Lawrence's introductory lecture; and you may form some notion of the spirit in which the scribe executes his task, when it is stated, that he commences by designating, as if with the finger of scorn, the first living British Surgeon, under the title of 'this gentleman.' He then proceeds to whine about the inexpediency of curtailing the number of quacks in the country; asks, in a tone of triumph, 'What would be thought of the state of that man's brain, who should open a school for the purpose of teaching all the medical sciences?' and concludes by stating, that 'the fact is, Mr. Lawrence's scheme in medicine, is that of Hunt and Cobbett in society at large—EQUALITY.'" "It is quite apparent that he hates the man, by speaking of him in such disrespectful terms,—that his motives are as mean as his arguments are powerless,—that while he asks, could any individual teach every branch of medicine, the Fadladeen himself proposes to criticise all books, from a treatise on periwinkles to a tome of metaphysics,—to be judge of every thing, from the painting of a Circassian's eyebrow to the merits of an epic poem. His efforts to answer that lecture, I suppose, are like the contortions of a maniac in a strait-waistcoat, all froth, foam, and jactitation. Is there one medical fact in the whole three hundred pages, which was not known to the profession before 1827?" "Not one; the contents of the work have been as familiar to every practitioner for the last six months, as 'household words.'" "It is, then, you conceive, a perfect blank?" "Not quite, for it concludes with a prize!" "Of what kind, and for what purpose?" "A complete set of the Medico-Chirurgical, to be continued for two years, with a perpetual insertion of the victor's name for the best report of a metropolitan or provincial hospital for the last quarter." "What do you think of entering the lists,—the Royal Infirmary is a fine field?" "Ha! ha! to write twenty pages for a complete set of Johnson's past and future botheration! to tell the truth in the report, and be excommunicated by the Senatus Academicus! or stand to be shot at in the King's Park until 'further notice!' Surely the moon is again at her work with poor Jemmy! 'O medici, medici, mediam pertundite venam.' But don't you think there is something fantastic in all we have been saying for the last half hour?" "To be sure there is; but consider the topic of conversation—Dr. James Johnson—for critics, you know, should be cameleons, and borrow a tint from every subject with which they may happen to come in contact."

SCOTUS.

Edinburgh, Jan. 20, 1827.

THE FRENCH SCHOOLS.

PARIS.—No. IV.

Il y a encore de quoi glaner.—FR. PROV.

HOPITAL ST. LOUIS.

AT the conclusion of the last letter, Alibert's classification of Tinea was spoken of, and it was intended to have added the composition of an ointment used in that complaint with great success, and which is, in consequence, become very popular throughout France. The formula is to be found in the last edition of Ratier's "Formulaire pratique des Hôpitaux Civils de Paris," together with a great number of others generally used in the Parisian Hospitals. It is as follows:—

Alicant soda, ʒiij;
Sulphuret of potass, ʒiij;
Hog's lard, ʒiij.

The soda, and the sulphuret of potass, should be very finely powdered, and then mixed with the lard. Alibert directs that this ointment is to be rubbed daily on the head, after the crusts or scabs have been separated by the application of emollient cataplasms. The hair is to be cut off as close to the scalp as possible, in order that the frictions may be made more complete. The head is to be covered with a brown paper cap. In the spring, cresses are plentifully prescribed; weak solutions of iron are also pretty freely given, and now and then sulphur.

Arsenic is frequently employed in the treatment of diseases of the skin at St. Louis, and principally in the cases of dartres. Alibert is an advocate for the stimulating local treatment, and almost universally orders the arsenical paste in the dartres rongeantes, and the dartres rongeantes scrophulouses (herpes exedens). Biett's favourite form

of using arsenic is the solution of the arseniate of ammonia, namely,—

Arseniate of ammonia, gr. viij ;
Distilled water, Hss.

Dissolve the salt in the distilled water, and add half an ounce of the spirit of angelica. Biett has used this form of arsenic since 1818, and although the solution of the arseniate of soda approaches very near to it in its effects, he gives the decided preference to the former. It is very useful in the *dartres squammeuses humides* ; according to the classification of Willan would be the chronic stage of ptyriasis, if they are not accompanied with too much inflammation. Like all other preparations of arsenic, this solution ought to be given with caution ; the doses should be gradually increased, and immediately omitted if any signs of irritation of the stomach should make their appearance. It should not be returned to until all thirst and disposition to nausea had completely subsided.

Alibert and Biett both employ arsenic very frequently in cases of chronic inflammations of the skin, and many cases have lately come under their care ; the patients began by taking twenty drops of the last mentioned solution, which quantity was gradually increased until one-eighth of a grain of arsenic was taken daily. This, then, is the force of arsenic, the most successful, and, consequently, the most used at this hospital.

Alibert's grand work on the diseases of the skin, in folio,* which was commenced in 1806, is now almost completed ; the tenth number has been printed, and the eleventh is in the press ; the whole work will be finished in twelve numbers. The price of each number is fifty francs. This work is inaccessible to the multitude for whom the small work in 8vo suffices,† 2 vols. 1822. It gives a summary of the disease of the skin, and is a very useful production to those who cannot afford to buy the large work. There are no plates with the octavo edition.

As an author, he has not confined his efforts to medicine, but has produced some poetical pieces, which are allowed to be well written. His late work, " *La Physiologie des Passions* ;" displays much original thinking and extensive reading ; it is universally praised by the French *savants*, and is placed on the shelves by the side of the " Anatomy of Expression" of Mr. Charles Bell.

Biett has written some very good articles in the Dictionnaire des Sciences Medicales, but has not published any thing more as yet ; it is generally thought that he is collecting materials for a work on the diseases of the skin, on those diseases particularly which appear to him defectively treated in the works of Frank, Willan, Bateman, and Alibert. He is a plain, steady, matter-of-fact man, and any thing coming from him may be relied upon. We cannot say so much of many of his contemporaries.

He gives his opinions very freely on the doctrines and practices of his contemporaries, and is well acquainted with the individual merits of the English and German pathologists. He very firmly opposes Alibert on several points, particularly the theoretical, but never gives judgment against him without stating his grounds in an open and manly manner ; but the production against which he most frequently protests, is Mr. Plumbe's " Practical Treatise on Diseases of the Skin ;" and he seldom gives a clinical lecture without pointing out some of the absurdities which it contains. Mr. Plumbe's name had been so frequently brought forth, that toward the close of the season some of the pupils began to laugh as soon as Biett began to criticise *Monsieur Plumbe*. Biett saw some gentleman smiling at the mention of Monsieur Plumbe, and made a full stop in the middle of the ward, to explain himself. " Gentlemen, you laugh because I speak of Monsieur Plumbe, but on my honour I assure you that I speak disrespectfully of no man, personally ; I have a duty to perform here, and a part of that duty is to caution you against the erroneous opinions of men who have appeared as writers on the diseases of the skin ; I know nothing of Mr. Plumbe ; but I know, and I repeat now what I have before so often repeated, there is no book more replete with error, or containing more incorrect representations of the subject to which it is devoted, than this work of Monsieur Plumbe ; in short, Gentlemen, its tendency is to carry back the study of dermoid pathology to the state in which it lay in the dark times of the middle age."

The merits of Richerand as an author are already well known ; his physiology established his reputation, and this has since been considerably extended by his " Nosographie et Therapeutique Chirurgicale,"

* Description des maladies de la peau observées a l'Hôpital St. Louis, et exposition des meilleures methodes suivées pour leur traitement. Paris, 1806.

† Precis theorique et pratique sur les maladies de la peau. 1822. 2 vols. 8vo.

He has published a very interesting work on the "*Histoire de Progrès recens de la Chirurgie*," in 8vo. It gives the merit of the invention of several new operations to their proper authors; and for so doing, has incurred the displeasure of some French critics, who, in common with the great part of their countrymen, are still the victims of a too jealous *susceptibilité nationale*.

Jules Cloquet has distinguished himself as an anatomist and surgeon, and is much respected by his contemporaries. His "Anatomie de l'Homme," which was commenced in 1821, in folio, is a good work; the description is clear, and the lithographic figures which accompany them are neatly executed; the principal objection to it is, its high price: it was intended to make the work complete in forty numbers, at nine francs each; but twenty-five have only appeared, and it is not probable that the undertaking will be executed. A curious story is told about this: Cloquet was induced to sell the copyright to a Count C——; and as no condition was made about publishing in a quarto form, Cloquet thought he was at liberty to do so, and accordingly commenced his quarto edition, which, on account of its smaller price, soon found purchasers. He has lately commenced a manual of anatomy* in quarto, with lithographic plates, which it is hoped will be completed in forty-five numbers, containing 250 plates, and 150 sheets of letter-press. The work will form three volumes; two of plates, and one of text for their explication. We have heard the Germans laughed at for producing *Handbücher* or Manuels of six and seven octavo volumes, and certainly Cloquet's Manuel of 3 vols. 4to, deserves to partake of the satire. Acupuncture, which was at one time so zealously practised here under Cloquet's direction is almost given up, and one may pass through St. Louis, or any other hospital, and not hear the word mentioned; so completely has passed away its high-sounding but ephemeral reputation.

* Manuel d'Anatomie descriptive du corps humain, representée en planches lithographiques. 4to. The first number was published in November, 1825.

LONDON MEDICAL SOCIETY.

JANUARY 22, 1827.

President, Dr. CLUTTERBUCK.

AFTER the minutes of the former meeting had been read, the President said, that as it was probable that the discussion which would ensue would be rather long, he should take the opportunity of briefly mentioning the history of a case of apoplexy which had recently fallen under his observation, as it would at the same time furnish the history of the preparation then on the table.

The brain which the President showed to the Society, was taken from the gentleman to whose case he alluded. There was a large extravasation of blood between the arachnoid membrane and the pia mater, which had extended over a considerable portion of the right hemisphere. The most singular fact in connexion with the history of the case was, that the voluntary power and consciousness were retained for an unusual time after the probable period of the occurrence of the extravasation. The gentleman was sitting at his dressing table shaving himself, when, all of a sudden, he felt as if something had burst in his head, became insensible, and, after a time, came down stairs to tell his wife of the circumstance. A medical man was sent for, who recommended bleeding, which was not complied with; he advised him to go to bed, and directed some medicine to be taken. The symptoms of apoplexy gradually increased, which were treated secundum artem. The hemiplegia which took place previous to death was, as usual, on the opposite side to that on which extravasation had happened. Dr. Clutterbuck was called in only in the advanced stage of the disease.

Mr. KINGDON, in referring to Dr. Barry's views of the venous circulation, remarked, that there appeared to be some little inconsistency in the arguments of those who opposed these views. They asserted, that the influence of the thoracic vacuum upon the venous blood, had been fully established by many, long before Dr. Barry had made his experiments; yet, in their opposition to this gentleman's theory, as to the effect of atmospheric pressure, they allowed little or no influence to this agent. At the previous meeting he was induced to express himself respecting the influence of the thoracic vacuum on the circulation, nearly in the following manner:—He thought that the influence of that vacuum, whatever that influence might be, and he was disposed not to esteem it of much importance, could exert itself only on compressible tubes; and, considering that

the hepatic veins in passing through the liver, had their exterior surfaces agglutinated to the structure of that organ, their calibres were kept *patent*, and therefore they approached to the character of incompressible tubes; and, further, seeing that they terminated in the inferior cava immediately before its passage through the tendinous opening of the diaphragm, to the sides of which opening that vessel was firmly connected, and therefore also rendered incompressible, he thought he had discovered an additional support for his conjecture, that the influence of the thoracic vacuum could only be exerted on those veins. He had, since the last Meeting, thought over the subject, and, like most other men who thought over what they supposed a novel idea, he could only find a further confirmation of its correctness. He was induced to think that this notion would receive some support from certain pathological changes in the heart and liver, and that the diseases of the two viscera might stand in some sort of mutual dependency in consequence of this arrangement. He had some conversation with Mr. Langstaff on the subject, than whom no man in this country was better acquainted with pathological anatomy, and he was pleased to accord with him in opinion. Mr. Kingdon then went somewhat more fully into a development of his hypothesis.

Dr. Barry next addressed the Chair, and spoke nearly in the following words:—Mr. President, In the last Number of The Lancet, there is a report of the proceedings of this Society at its last meeting, and as that report almost entirely agrees with the minutes which I have just heard read, I must consider it authentic. Mr. Spry, in the observations attributed to him in this report, endeavours to prove, 1st. That the doctrine of a thoracic vacuum influencing the venous current, did not originate with me; 2dly. That this influence must be very inconsiderable, as the blood could be returned to the heart without its assistance. As to the first of these propositions, since Mr. Spry seems to think the property of the vacuum doctrine worth the labour of so much literary research as he has displayed, I, on my part, do not feel disposed to allow my share in that firm to be frittered away. I have said in my book, and I now repeat, (continued Dr. Barry,) "That a *vague unauthenticated* notion, that the return of the black blood to the heart is, in some undefined way, influenced by *suction*, may be traced back as far as the time of Harvey." I contend, Sir, that I am justified in the use of the term *vague*, because neither the originator of this notion, nor the epoch of its first adaption, can be distinctly pointed out; and because no two of those who appear to have entertained the notion of suction, explain, in the same way, the mechanism by which it affects the venous blood. Galen seems to allude to it, when he talks of the "*vis pulsifica*" of the heart. Vesalius distinctly mentions it, when he attributes the form and the office of the *cucurbitula* to this organ. In the days of Harvey, the doctrine of suction influencing the filling of the heart, was commonly believed. "Quod vulgo creditur," says this great man. Boerhaave considered the whole animal an hydraulic machine, of which the heart is the piston. Dr. Wilson, of Newcastle-upon-Tyne, with many others, and amongst these M. Magendie, in the first edition of his physiology, conceived that the fibres bounding the cavities of the heart, possess an active power of dilating these cavities, with a force equal to that of their contraction, and, therefore, that the heart is, *per se*, alternately a sucking and a forcing-pump. Dr. Carson gives a diagram of the heart, to prove that its fibres are so disposed, that their *relaxation* produces a tendency to a vacuum in the cavities of this organ. This physiologist also states, that the lungs are *constantly* exciting a "resilient power" to separate their surfaces from the costal and mediastinal pleuræ, thereby producing a tendency to a vacuum in the thorax, and around the heart. Now, Sir, there is not one of these various theories *authenticated* by experiment. To each of these notions may be applied the expression of Haller, "Vaga cogitatio—in nulla fundata experimento."

I trust, Sir, that I have now shown that I was not so culpable as Mr. Spry would have it believed, in writing the paragraph which he quotes from the preface to my book, relative to the vagueness and want of authenticity of the notions hitherto entertained as to the influence of the thoracic vacuum. Mr. Spry blames me for not quoting largely from the authorities he has enumerated. To this I reply, that my experiments were made in 1824; I have given names, dates, circumstances. A gentleman is now in this room who assisted at my first experiments. The authority quoted by Mr. Spry, the second edition of M. Magendie's Physiology, describing the experiment that approaches the nearest to mine, was published in 1825. I read my Memoir at the Institute of France in presence of M. Magendie. In the preliminary observations of that Memoir, I have stated that I should avoid all polemical citations, because I had room only for facts. When I published this memoir in England, I felt, and I feel now, that I should not have acted honestly, if I gave a syllable to the public under the sanction of the names of Cuvier and Dumeril, which had not formed a part of the subject of their luminous report. Besides, quoting mere speculative theories, endless and bodiless as the his-

tory of a dream; "laborious figmenta," would be an unprofitable employment, whoever might be the author, or whatever might be the subject of these theories. As to the doctrine "of the thoracic vacua influencing the venous blood, not having originated with me," I never claimed, said Dr. Barry, any other merits than what might result from having been the first, the very first, the only person, who proved by experiment the exact nature of the mechanism employed to form the vacua in the sanguisugnous cavities of the mammalia. It was upon this ground alone that I threw down the gauntlet in the preface to my book, when I said, "that no man, either living or dead, had even imagined my experiments before I executed them." This was the gauntlet which Mr. Spry should have taken up. Mr. Spry has employed much ingenuity and research in his endeavour to drive me from a position which I never occupied, to strip me of an honour which I never could have claimed. If Mr. Spry found the position I had taken up with regard to the *novelty* of my experiments, and their consequences, impregnable, he should have candidly confessed it, and then he might have attacked the originality of the theory itself, which he must have known *I had never claimed*. Mr. Spry undertakes to prove, that the influence of unresisted atmospheric pressure on the return of the venous blood, must be very inconsiderable. Yet he says, "Magendie had fully established the concordance of the increased movement of the venous current with inspiration." Upon looking into Magendie, I find that he applies the term "*grande influence*" to the effect of the expansion of the chest upon the progression of the blood in the veins. That the pressure of the atmosphere thus modified, "is capable of arresting the circulation altogether." This surely might, without much impropriety, be called a "*mighty agency*." The words of M. Magendie himself, in the very page quoted by Mr. Spry, will show that *he* had no very accurate notions of the effect of the expansion of the chest on the sanguisugnous cavities. "When *chance*," says M. Magendie, "produces a coincidence between the dilatation of the thorax and auricle, the motion of the blood in the jugulars is regular." For the explanation of this and all the other circumstances connected with his experiment, he refers to Haller, Lemure, and Lorry. Now, Sir, I have proved by positive experiment, that the auricle (not the appendix,) and the thorax always dilate and contract together. I have introduced tubes, both through the anterior and posterior cava, into the sinus venosus of the right auricle in dogs, and the liquids in communication with these tubes invariably ascended during the whole period of inspiration, and never at any other time. I say, Sir, that Mr. Spry had no right to designate this direct fact as a "matter-of-course inference" This single experiment, if there never had been another made, must refute every thing that he has said about the filling and emptying, and relaxation of the heart, and the old tale of the two-ounce jets of blood from the left ventricle, which he assumes as the basis of his next argument. By the experiments made and related at my late Lectures, I have proved that the heart is always full, and always in a forced state of distention, even when the animal has been killed by bleeding. In the second and third experiments given in my book, I have proved that there is no tendency to vacuum between the pulmonary and costal, or mediastinal pleura, during expiration. So much for Dr. Carson's theory. With reference to Mr. Spry's arguments founded upon the assumption of two ounces of blood being thrown from the left ventricle into the aorta at each contraction, which would render *a sixty-ounce reservoir* necessary in case of suspended inspiration; does Mr. Spry recollect, that within the thorax there is at all times the venous blood of the whole torso, the blood of the azygos and demiazygos, of all the intercostal veins? Does he recollect, that during the voluntary supension of respiration, the glottis is shut; that the thorax neither collapses nor expands, and that the locomotion and contraction of the heart produce vacua, towards which the unresisted pressure of the atmosphere will force the blood of the nearest veins? The filling of the veins by their corresponding arteries at all distances from the heart, and the simultaneous rush of the contents of these veins towards the heart, when the thorax is expanded, are very different things. In short, this simple fact is a sufficient reply to all the objections that have hitherto been made to the inspiratory movement of the venous blood, viz. *that a cavity in the act of being diminished in all its diameters, can receive no addition to its contents, and that when this cavity is in the act of being similarly enlarged, all communicating liquids must be pressed towards it.* Mr. Spry has drawn some arguments from comparative anatomy, and if I understand that gentleman rightly, he said that the enlargement of the posterior cava in the cetacea, the seal, &c., is for the purpose of receiving the blood, which the right side of the heart cannot transmit to the left, the pulmonary circulation of the animal being impeded during his immersion. This abdominal reservoir, therefore, (according to Mr. Spry,) continues to be filled by the *vis a tergo*, while the animal remains under water. When the whale, after having continued his full period below,

comes to the surface, makes one inspiration, is struck by a harpoon and dives, he must go down with his venous reservoir *nearly full*. What must, then, be the capacity of this reservoir, if it be able to contain all the blood returned during the two periods by the vis a tergo? From whence could the left heart derive its supplies, if the returning current were to be turned aside, and retained in the enlargement of the abdominal cava?

All these difficulties, and a thousand others, are avoided by conceiving that this reservoir is filled at the surface by the operation of the thoracic pumps, and that as soon as all the blood it contains has been forced into the thorax by the pressure of the water around, the animal is again obliged to come to the surface to fill it a second time, by one or two strokes of his thoracic pumps.

I now come to Mr. Spry's four experiments: these four (continued Dr. Barry with some warmth) I contend constitute but one experiment, and that one is a literal copy of the experiment, so often repeated by Harvey, to prove that there is a *vis a tergo*, and that the venous and arterial currents run in opposite directions. Harvey compressed veins at various distances from the heart, and showed that blood will flow when the vein is wounded below the point compressed. Had Mr. Spry entertained any doubts as to the direction of the venous current, his experiments would tend to dissipate them, but they leave the question of the thoracic vacuum entirely untouched. There are some curious circumstances connected with the experiment upon the dog, which appear to have escaped Mr. Spry's notice: when the dog was held perpendicularly by the fore paws, his chest open, and his cava tied, and wounded behind the pericardium, there were *three* powers employed in forcing the blood through the opening in the vein: 1st, the vis a tergo; 2dly, the weight of a column of blood acting through the azygos, from the tip of the fore paw; 3dly, the contents of the stomach and intestines compressed against the mesenteric veins, by the tension of the abdominal integuments forcing the blood of the porta into the thoracic cava. Hence the blanching of the intestinal veins and liver. In these experiments Mr. Spry appears to have forgotten the very just and beautiful observation of Dr. Carson—"That whoever admits into the regions occupied by the heart and lungs, the vision or the touch, must have already admitted an influence destructive of the machinery; and like the school boy, inspects the cavity of the drum into which he had cut, for the purpose of discovering the origin of its sound." Why, instead of laying open the thorax, did not Mr. Spry make the experiment which he (Dr. Barry) had made, and introduce his hand into the thorax of the horse, and in that way satisfy himself that the blood did return through the cava only during inspiration? Why, he would ask, did he not satisfy himself on that point? Had he done so, he would have felt the rush of venous blood through the cava during inspiration only, and he would have felt the progressive dilatation of that vessel, and the sinus venosus during that act.

Mr. Lloyd stated that he had made some experiments to determine how far the lungs possessed the power of adapting themselves to the varying dimensions of the chest, but more particularly during inspiration, in order to ascertain what was the amount of vacuum that could possibly be formed between the parietes of the chest and the surface of the lungs, and to ascertain the influence of inspiration and expiration upon the pericardium, and consequently on the vacuum supposed to be formed there. He had seen all Dr. Barry's experiments repeated before he began to make his own, and we understood Mr. Lloyd to say, that although he was led from these experiments to admit that there was a tendency to the formation of a vacuum in the thorax during inspiration, that his own experiments had led him to a different conclusion, and so far from the dilatation of the chest assisting in forming the pericardiac vacuum, the pressure of the lungs on the surface of the pericardium tended to prevent it. Mr. Lloyd, after describing at some length the relative localities of the thoracic viscera, insisted that the lungs in their expansion must press the pericardium in upon the heart, and destroy all tendency to vacuum within that bag during inspiration.

Dr. Barry replied by observing, that one fact was worth a thousand speculations. Experiment, he said, proved the very converse of Mr. Lloyd's theory to be the fact. He referred to the second and third experiments in his book. Dr. Barry here took an opportunity of alluding to the arguments drawn by Mr. Spry from comparative anatomy. Cuvier had confined the reservoir attached to the posterior cava to the *phoca* in the mammalia, and the *plongeon* in birds. But if this reservoir does exist in whales, and if it serves the purpose of receiving the blood sent up by the vis a tergo, and which cannot be taken into the thorax during the immersion of the animal, (the circulation in the lungs being impeded, as stated by Mr. Spry,) he would ask, from what source does the left side of the heart receive its supplies, if there be no reservoir in the great pulmonary veins. After the reservoir of the posterior cava has been filled by the animal's longest stay under water, and when, after making a single inspiration at the surface, he dives again, what becomes of the ad-

ditional blood sent up by the vis a tergo? What must be the size of the reservoir, when the whale comes to the surface a second time? All these difficulties would be avoided, by conceiving that the enlargement of the posterior cava behind the liver is intended to contain an additional supply for the right heart, while the animal is under water; and that when exhausted, it is filled again at the surface, by a single expansion of the thorax.

Mr. Spry begged leave to repeat the assurance he had before given the Society, that he entertained no other feelings than those of the greatest respect for Dr. Barry, and that it was his only object to arrive at the true merits of the question. It afforded him pleasure, on retracing the long speech of Dr. Barry, to observe, that no one point which he had taken to prove that the originality of the thoracic vacuum theory did not belong to Dr. Barry, had been successfully assailed by that gentleman; nay, he had himself admitted that he had no claim to it. Dr. Barry had needlessly carried the attention of the Society back to remote periods of medical history, to prove that the notions entertained respecting the nature of that vacuum were, with the opinions of the moderns, "vague and unauthenticated;" but that the merit of having pointed out the mechanism of that vacuum belonged exclusively to him, or as Dr. Barry had expressed himself, "the mechanism *was never pointed* out by which nature applies the mighty agency of atmospheric pressure to the veins, and connects, as cause and effect, the expansion of the chest with the afflux of the centripetal fluids to the heart." He was surprised at the reading of this sentence in the preface to Dr. Barry's book, but he was the more surprised to hear Dr. B. now repeat the same sentiments, because he thought it possible that Dr. Barry might not have seen Magendie's book at the time he wrote his preface. It was his object at the preceding meeting to show, by a careful mention of dates, how gradually the thoracic vacuum theory had arrived to something like completion; the vacuum was first supposed to be confined to the heart, it was then connected with respiration, and lastly, the *act* of respiration concerned in its production, namely, *inspiration*, was pointed out; and if it were done by no other man, it had been done sufficiently by Magendie, who had thus expressed himself:—"*During the dilatation of the thorax*, the blood of the vena cava superior and of the vena cava inferior, and by degrees that of the other veins, is drawn toward the heart. The *mechanism* of this aspiration is similar to that which draws the air into the lungs; it is, so to speak, *an inspiration of venous blood.*" Here, Mr. Spry contended, the mechanism was as clearly described as words could describe it; the flow of venous blood to the heart was ascribed to the *same cause* as forced the air through the trachea during the expansion of the chest; or, in Dr. Barry's own words, "it connected, as cause and effect, the expansion of the chest with the afflux of the centripetal fluids to the heart." The second edition of Magendie's Physiology, which contained the paragraph read, was published at Paris in the month of *March* 1825, and the report of the Institute on Dr. Barry's paper was not made until the 29th of *August* of the same year. Consequently, his "Recherches Experimentales" could not have been published at Paris until after that time. Dr. Barry had said, that he made his experiment of introducing a flexible cathether into the jugular vein of a horse in Oct. 1824; of this he (Mr. Spry) entertained no doubt, but he had the best authority for stating that Magendie had performed the same experiment, on the same vein, *four years* ago. Magendie took to himself no credit of originality for this, but referred to Haller and Lorry as having before noticed the exact correspondence which was sought to be established between the act of inspiration and the flow of venous blood to the heart. He (Mr. Spry) would trouble the Society with only another passage from Magendie's work on the subject, it was this, "The explanation of these phenomena, as it has been given by Haller and Lorry, is very simple and satisfactory at first sight. When the chest *dilates* it draws up the blood of the vena cava, and by degrees that of the veins which open into them. The *mechanism* of this aspiration is very nearly similar to that by which the air is drawn into the trachea." Was there any thing *vague* and *unauthenticated* in these passages? Had not Magendie said *all* he could say on the subject in the plain but conclusive language he had employed, and had he not performed experiments to prove the fact? Yet Dr. Barry had said, that "the mechanism had never been pointed out, and that no man, whether living or dead, had even *imagined* his experiment but himself." Dr. Barry had excused himself for not mentioning Magendie's book at the Institute, because he wished to avoid polemical discussion; because he thought that all papers read to learned Societies should be as short as possible; and that if he had again to publish an English edition he should do the same thing; that was to say, he should altogether omit to mention what Magendie had done on the subject. He (Mr. Spry) would say, that if he did so, he would deservedly merit the censure which had been already expressed, since it was a duty which every man owed to his own reputation and to the public, to put the public in possession of whatever was of importance connected with

the investigations to be undertaken. Dr. Barry had said, that he had never claimed the merit of originality; but whether he had or not might be best learned from his own preface. If "the mechanism had never been pointed out" by any other man than Dr. Barry, he was entitled to the merit of having pointed it out; and if it had been before pointed out by any other man, the merit of pointing it out could not belong to Dr. Barry. Dr. Barry had said, that the experiment on the horse was only the counterpart of the experiment of Harvey, to demonstrate the circulation of the blood; but Harvey had never said in what position the animal was placed when he performed the experiment. It was the design of the experiment on the horse, as well as of that on the dog, to allow *gravitation* to act on the returning columns of blood, and although the weight of the returning column of blood was allowed in each case to operate freely and fully, the blood did return up to the place on which the ligatures were applied. Dr. Barry had asked, why he (Mr. Sapy) had not thrust his hand into the thorax of a horse and *felt* that the blood did return through the vena cava only in inspiration? He should answer, that he did not do it because he could have the evidence of another sense under more favourable circumstances. He would ask, on which sense the greatest reliance should be placed in the present case, on the feeling or the sight? It required no inconsiderable effort to pass the arm into the thorax of such a powerful animal as the horse; and to make that opening through which the arm was introduced air tight, as Dr. Barry had imagined he had done, a great degree of pressure must have been made on some part of the arm, pressure sufficient to take away all delicacy of sensation in the fingers. He had by no means overlooked the beautiful remark of Dr. Carson; but it appeared that Dr. Barry had forgotten it when he asked why the experiment of thrusting the arm into the thorax of the horse had not been performed; he appeared to have forgotten that he had even by that experiment brought himself within the scope of Dr. Carson's remark, since the *touch* was considered as fatal to the machinery as the *sight*. He preferred the evidence of his vision, therefore, to that of the touch under these circumstances, and the more so as it was equally available by bystanders. Fact was better than theory; and as he had evidence to adduce so unobjectionable as ocular demonstration in support of his second proposition, he should still consider that he had succeeded in showing that the influence of the thoracic vacuum on the return of the venous blood must be very *considerable*, as the blood did return to the heart without its assistance; and that nothing that Dr. Barry had said had proved his title to the merit of originality even for his experiments.

Dr. Macann said, that his name having been mentioned by Dr. Barry, he felt himself called upon to say a few words in explanation of the facts to which that gentleman had alluded. With this object in view, the shortest and most satisfactory course perhaps would be, to give a simple narrative of the circumstances connected with the institution of Dr. Barry's first experiments, and the preparation of his original memoir, from which it would appear that the charge of Dr. Barry having borrowed any thing from the work of M. Magendie, was not well founded. In the month of August 1824, (continued Dr. Macann,) Dr. Barry did me the honour of laying before me, in Paris, a memoir, which he had then composed, "On the Motion of the Blood in the Veins." The object of this memoir was to show that this motion was, or ought to be, in a great measure, if not entirely, influenced by atmospheric pressure. This memoir, however, was entirely hypothetical, that is, it consisted in a chain of reasoning founded upon abstract principles, and tending to show that the motion of the venous blood must be so influenced, if these principles were well founded. After having carefully examined this memoir, I remarked to Dr. Barry, that if these principles were well founded one experiment would establish the conclusions he had drawn from them. This experiment was performed in my presence, as stated in Dr. Barry's book, and by it his anticipated conclusions were verified. There may be some dispute hereafter as to the priority of claim to the abstract theory, but there can be none as to the novelty and originality of the experiments. It had been long known that water would ascend in the pump, and mankind had availed themselves of this knowledge; but the certainty of the fact does not deduct from the merit of the man who first demonstrated that the water is forced to ascend in the pump by the pressure of the atmosphere. From the long intimacy which has subsisted between Dr. Barry and myself, I can affirm that at the time he wrote the memoir I have mentioned he had scarcely heard of Dr. Carson's book, and that the first copy of it he had ever seen was sent by me to him from London in 1825. With regard to the memoir read before the Institute, I took the liberty of suggesting that it would be unnecessary to detail all the steps of the chain of reasoning by which he had been led to his experiments, and that facts should be principally attended to. The whole memoir was accordingly re-written, and as it was intended to be read before a learned body it

was necessarily short, and admitted of no room for quotations. Dr. Macann then proceeded to make some observations on the fallaciousness of the conclusions drawn from the experiment with the bellows; he said, that one fact had particularly struck him, it was this: the trachea in the human subject was larger than any vein, yet in Mr. Searle's bellows the tubes to represent veins, which were four in number, were all larger than those representing the trachea. This caused his principal objection to the apparatus.

Mr. KINGDON would ask one question. Did Dr. Macann mean to state that the rima glottidis was larger than any vein in the body, that it was larger than the vena cava inferior, for example, where it passed through the tendon of the diaphragm?

Dr. WOODFORDE stated that the bellows of Mr. Searle was furnished only with three tubes to represent veins, and that one of them was decidedly smaller than the nozzle of the bellows representing the trachea.

Dr. MACANN replied that the bellows he had seen at the Hunterian Society was of the description he had named.

Dr. BARRY said he had to apologize for again intruding on the attention of the Society. He would remind the Society, that all the experiments which had been made by persons who felt opposed to his doctrine, had confirmed his statement, that there was some vacuum found in the chest during inspiration; this had been proved by the experiments of Mr. Ellerby, Dr. Davis, and others; in short, no man had denied it; but it was contended, that the influence of that vacuum was *inconsiderable* in its action, because the blood could return to the heart without its aid. Mr. Spry comes to a conclusion which I am sure he will himself, upon reflection, acknowledge not to be very logical. He hopes "that his experiments would suffice to show, that the thoracic vacuum, the existence of which, to a certain extent, he did not mean to deny, exerted little influence on the return of the venous blood, as it was very evident that the venous blood could be brought back to the heart by other powers." It would be just as logical to conclude, that because the bull and the mule (in Miss Edgeworth's humourous description of King Corney's ploughing-team) could drag the plough a few yards without the assistance of the old horse, the latter could give no assistance if again tackled on by a fresh hay rope. King Corney's ploughman, however, had too much logic to come to the conclusion, and the old horse was accordingly added once more to the heterogeneous team. I hope for the honour of the very little mathematical demonstration and rational induction still left to our conjectural art,—nay, for the well-merited reputation of Dr. Clutterbuck himself, as a medical philosopher, that he did not make the declaration *ex cathedrâ*, which in THE LANCET he is stated to have made, viz. "That from what he had heard that evening, he considered that the influence of the thoracic vacuum on the circulation was almost nothing, and that it would be unjustifiable cruelty to make any further experiments on the subject." But what has the learned Doctor heard? Why, he heard that veins had been tied at various distances from the heart, with the thorax open, and the thorax shut, and that when these veins were punctured below the ligature, blood had flowed. So, because blood flows under these circumstances, it is proved *to demonstration* that atmospheric pressure, modified by the organs of the animal, cannot have any influence on its current, under any circumstances. In short because after the bull had eaten the hay rope that connected the old horse with the plough, he and the mule could still drag it, after a fashion, it would be unjustifiable cruelty to try whether the old horse, if tied on again, could give any assistance or not. Dr. Barry concluded, by inviting Mr. Spry to imitate the examples of Mr. Ellerby and Mr. Searle, who had repeated his principal experiments, and who (though opposed to his inferences) had acknowledged their correctness with a manliness and candour which did them honour.

Mr. SPRY said that there were a few remarks made by Dr. Barry, which required reply. Dr. Barry had now given up all claim to the originality of the theory, but he contended for the originality of the experiments made to show that the theory was true. Dr. Barry might have saved himself that trouble as the same thing had been done before him by others; Magendie had described the mechanism by which the vacuum was found *before* him, and had confirmed the accuracy of the opinion of his predecessors by actual experiment. So that although *the fact* for which Dr. Barry's experiments were made to establish, was long since admitted; yet, *for* these experiments, brought to bear upon an admitted fact, Dr. Barry claimed originality, and no small share of glory. What would be thought of the pretensions of a man who, because he had made some *experiments* to demonstrate the circulation of the blood, different perhaps from any before made, should turn round and claim the *discovery* of the circulation? Or who, being driven from such a claim, should say, if Harvey discovered the circulation, he had made *experiments* to show the fact different from those made by Harvey, and therefore entitled to a share of the glory of the discovery? Dr. Barry had objected to the quantity of blood sent from the ventricle; he, (Mr. Spry) stated two ounces on the authority

of Haller, Blumenbach, Richerand, and other most distinguished physiologists; and until Dr. Barry had made some experiments or adduced some sort of evidence to show that that quantity was not expelled at each contraction of the left ventricle, he should still assume that it was. If Dr. Barry wished to know on what authority the peculiar structure of the animals named had been mentioned; he should refer him to Meckel, and to Carus, or he believed it had also been quoted by Fyfe in the 4th vol. of his Compendium of Anatomy, which might be deemed a pretty correct abstract on the subject. Dr. Barry had laid great stress upon the formation of the vacuum in the pericardium, But what was to become of that vacuum in cases where the inner surface of the pericardium was glued to the surface of the heart? Dr. Barry must have recollected the experiment made by Mr. Searle on the rabbit, as shown to the Hunterian Society, in which the pericardium was opened by removing a portion of the sternum, the finger was introduced in absolute contact with the heart, and of course atmospheric pressure; yet the animal had lived for *many hours*, and the circulation therefore must have continued. He, Mr. Spry, did not deny, as he had before stated, the existence of the thoracic vacuum, or that it might not exercise some influence on the return of the venous blood, but he would contend that that influence had been proved to be *inconsiderable*, and undeserving that rank which had been assigned to it among the auxiliary forces of the circulation.

[Some further debate ensued, but as no matter of argument was involved, we have not room to notice it.]

Dr. Clutterbuck said that he had been induced to express his opinion on this subject on a former occasion, and he felt it right again to do so. He thought that it was clearly established, by the experiments of Dr. Barry and of those who had repeated them, that there was some sort of vacuum formed in the chest during inspiration. He had never questioned the accuracy of Dr. Barry's experiments, but he dissented from the inferences which Dr. Barry had drawn. It was necessary to be borne in mind, that as all experiments must be attended with considerable pain to the animal, increased efforts were made; so that it was probable that no experiment could ever determine the natural condition of the chest in respiration. He did not mean to deny that a vacuum to a certain extent might exist, and prove of some use in the circulation under certain circumstances; as, for example, when great efforts were made by the animal: but he could not help thinking that its influence was neither so constant nor so important during ordinary respiration as Dr. Barry had supposed. He had already expressed his opinion that sufficient evidence had been adduced to justify that conclusion.

Dr. Barry said that he had already gained much ground. It was now allowed on all hands,—1st, That his experiments were correct; 2dly, That atmospheric pressure does exercise *some influence*, on the progression of the venous blood. It only remained to establish the amount of this influence. This latter task he thought he had already accomplished, by the application of the barometer as described in his lectures. Dr. Barry considered Mr. Searle's very ingenious experiment with the bellows-thorax as a confirmation of one of his leading inferences; viz. "That it is the repetition of inspiration which regulates the velocity of the blood returning to the heart."

TO PHYSICIANS.

(CIRCULAR.)*

A private Association of Physicians in the Metropolis has been occupied more than a year in maturing a plan for improving the state of the Medical Profession, upon principles founded on the broad basis of public utility. They now offer it for the approbation and support of the great body of the Faculty throughout the British dominions, as well as of the Community at large.

That in which they are engaged is entirely a common cause, in which all are equally interested. The individuals who now address their brethren, will therefore not make any personal applications: nor will they even avow themselves, until a number of the faculty, in town and country, sufficient to form a highly respectable and efficient body, shall have signified their intention of co-operating with them. In raising, not unworthily they trust, the standard of professional improvement, and indicating a safe rallying point for *unprotected* physicians, they have rendered the labours of those who may now join them comparatively easy.

It being particularly desirable, at the commencement of an undertaking of this magnitude and importance, to ascertain the sentiments of the faculty, it is earnestly requested that, on receiving a copy of this circular, they will communicate their opinion upon it, either directly, or through some other physicians. They will please, in order to save the postage of many letters, to transmit their communications in franks, or otherwise free of expense, addressed to

* This is the Document mentioned at pages 359 and 555 of our present volume.—Ed.

the Secretary of the Faculty of Physic in London, and to the care of Messrs. Longman and Co., Booksellers, Paternoster-row. Each physician receiving a copy of this circular, is requested to communicate it to others; and afterwards to favour the Association with their collective sentiments.—Such as do not receive copies will impute the omission, in the absence of a list of the faculty, to ignorance of their address. In order that such omissions may be supplied, information respecting the names of doctors in physic, resident in their own or the neighbouring towns, is requested to be transmitted in the answers returned by physicians receiving this circular. Suggestions of all kinds will be favourably received and attentively considered. This Association have only further to add, that since the Surgeons and General Practitioners have respectively formed unions, have already presented Petitions to the Legislature, and are proceeding to adopt other measures in support of their particular interests; unless the physicians also form a similar union in support of their own rights and those of the public, bad as things now are with this higher brach of the medical profession, it is very far from being improbable that they may still become much worse.

THE FACULTY OF PHYSIC IN LONDON.

A few physicians, who had long observed with regret the detriment to the public, and the debasement to the medical profession, occasioned by the artificial, forced, discordant, disproportionate, and fluctuating state into which its different branches had fallen, did, on the 27th of July 1825, constitute themselves into the nucleus of a proposed Association, for the purpose of ascertaining, promulgating, and endeavouring to establish such an organization of that profession as is calculated, by emulation and competition, to bring into activity the greatest sum of talent, knowledge and integrity; and thereby to ensure to the community, in the highest attainable degree, the preservation of health, and the prolongation of life, as well as to the members of the profession themselves the greatest sum of stability and respectability, of which their condition is susceptible.

At various meetings subsequently held in pursuance of their object, they agreed upon the following principles and rules:—

PRINCIPLES OF THE ASSOCIATION.

In the organization of the different branches of the medical profession, the main object of consideration, as in all other departments, ought to be the good of the community; by consulting which, also, the respectability of their own members will be best insured.

From the almost fortuitous manner in which laws and institutions have for the most part arisen, and the little share which the public voice has generally had in framing them, in those periods of comparative ignorance in which they originated, it has necessarily happened that they have been much more calculated for the convenience and aggrandizement of the members of the professions concerned, or their separate branches, than for the benefit of society at large. These observations hold good of the different branches of the medical profession more than of any other; and of that profession, in the British dominions, more than in any other nation.

It is essential to the objects in view, that there should be no limitation to the number of any of the branches of the medical profession, excepting what is imposed by the demand; that there should exist complete emulation and unrestrained competition among their members; that the public should have a free choice of their medical attendants; that the tests of fitness should be proper and uniform; that examination should be conducted upon determinate principles, and not by bodies having an interest in the issue; and that universities should be regulated with a view to a correct and efficient medical discipline and instruction.

Is is an obvious deduction from the clearest principles of our common nature, that those members of any profession who benefit by its mal-organization, will not only not aid in, or consent to, but will on the contrary, by every means in their power, oppose the improvements which would deprive them of their exclusive privileges; and it follows as a corollary from this proposition, that the representations of parties interested in the perpetuation of existing abuses in this profession should be listened to with distrust.

That medical constitution best adapted to maintain the welfare of the community, and the respectability of the profession, we think can best be obtained by free and frequent discussion, by an union of persons, who, from talents and education, are competent to a thorough investigation of the subject, and, from their position in society, have no interest in perpetuating delusion or abuse. In the words of Addison: "An honest party of men, acting with unanimity, are of infinitely greater consequence than the same party aiming at the same end by different views." There cannot be a doubt that an extended and numerous union of physicians, affiliated throughout the cities and counties of the British dominions, would speedily be able to produce conviction in the public mind, in the Legislature, and in the Minis-

ters of the Crown, of the utility and necessity of remodelling, upon correct principles, the various branches of the medical profession in the United Kingdom. An Association of the Graduates of Universities, too, it is obvious, can in the mean time establish and maintain themselves in the independent and honourable exercise of their profession, in virtue of the rights conferred by their diplomas, and in opposition to the usurped authority of medical incorporations, and every other species of undue interference. For although it is true that, against such usurpation and interference, every doctor of physic may, to a certain degree, singly maintain his individual rights, it is only by union on an extended scale, that the members of the faculty can permanently obtain complete security and protection.

RULES.

ELIGIBILITY OF MEMBERS, &c.

All graduates in medicine practising as physicians are eligible, in virtue of their diplomas, to become members of "The Faculty of Physic in London," without distinction of university or country. The proceedings of this Association, it is proper here to state, are intended to be wholly prospective.

The diploma of the candidate for admission into the Faculty of Physic in London being verified and registered in the office of the Association, upon subscribing to their principles, he is of course entitled to be enrolled as a member.

When any question arises rendering eligibility or the exceptions doubtful, or in any other case of difficulty, the doubts to be resolved by the ballot, at the next subsequent meeting, or any more convenient time.

Members in the country to be at liberty to vote by proxy.

Other rules to be enacted as the occurrences arise which may seem to call for them; holding always in view the maxim, that the principal security and ornament of an association, founded on just principles, consist in the absence of all unnecessary restrictions.

In the course of the proceedings and discussions which have taken place, it became evident that some more efficient means than had yet been contemplated of informing the faculty and the public respecting the nature and extent of the mal-organization of the different branches of the medical profession, were indispensable to the success of the object in view within a reasonable period. The task of investigating the actual state of these different branches, but more especially of that of physic, as well as the causes and consequences of their palpable mal-organization, was therefore deputed to a portion of the associates: and the results of their labours have since been published, by Messrs. Longman and Co., in a work entitled "An Exposition of the State of the Medical Profession in the British dominions, and of the Injurious Effects of the Monopoly by Usurpation of the Royal College of Physicians in London." To that work reference may be made, as illustrative of the principles and views of the Association. But, in the mean time, it has been thought right to give in the present Circular its principal heads, under the form of resolutions, in order that no person who joins them may be able to allege that any of the principles of the associates have been either misrepresented or concealed, and that all who read them may be led to a due sense of the utility and necessity of a thorough medical reformation:—

Resolved,

1. That, in London, there are 174 physicians, being, on a population of 1,200,000, one to 7000 inhabitants; whilst, in Paris, there are 600, being, on a population of 800,000, one to 1300 inhabitants. Consequently, in London, the ratio of the physicians to the population is to that of Paris but as *one to five*.

2. That, in London, there are 1000 surgeons, or one to 1200 inhabitants; whilst, in Paris, there are only 128, or one to 6000 inhabitants; the proportion in London being to that in Paris as *five to one*.

3. That, in London, there are 2000 *practising* apothecaries, or one to 600 inhabitants; whilst, in Paris, there are only 180 *dispensing* apothecaries or pharmaciens, or one to 4450 inhabitants; the proportion in London being to that in Paris as *seven to one*.

4. That, in London, there are besides upwards of 300 chemists and druggists, (the *dispensing* apothecaries or pharmaciens of other countries,) being in greater proportion than that class in Paris, and quite sufficient, under a good organization, to supply all the inhabitants with drugs. This branch has, in this country, under the prevailing system, been called into existence by the wants of the people, within the last half century.

5. That, in London, the physicians are to the surgeons as one to six; to the apothecaries, as one to twelve; to the chemists and druggists, as one to two; to all of them united, as one to twenty; whilst, in Paris, the physicians are to the surgeons as five to one; to the pharmaciens, as three to one; to both united, as two to one; the ratio which the physicians in London bear to all the other branches of the medical profession, being to that in Paris as *one to forty*!

6. That since in Paris no undue restrictions exist, in respect to the different

branches of the medical profession; and since their relative proportions to each other, and to the population, are similar to those which obtain in other cities, where no undue restrictions prevail, it is to be presumed that these proportions are the result of the necessary adaptation, under freedom from undue restraint, of the supply to the demand.

7. That since there cannot naturally be, in any state of society, such a relative demand for medical, surgical and pharmaceutical aid, as is indicated by the existing relative proportions of the different branches of the medical profession in London; and since similar relative proportions do not obtain in cities where no undue restrictions exist, it follows that the artificial, forced, discordant, disproportionate, and fluctuating state of the different branches of the medical profession in London, is mainly the result of the monopolies which relate to these branches.

8. That the healthy state of the different branches of the medical profession in Paris, and throughout France generally, being admitted to depend upon the due adaptation of the supply to the demand; and their unhealthy state in London, and throughout England generally, to depend upon restrictions which prevent that due adaptation; and the ratio which, under these circumstances, the physicians in London bear to the other branches of the medical profession, being, relatively to Paris, as *one to forty*; it follows,—the wants of the inhabitants of these cities, in respect to the aid of these different branches, being supposed to be equal,—that the excess of the other branches over that of the physician in London, is as FORTY TO ONE!

9. That according to the proportions which exist in Paris, there should be in London, 900 physicians, 170 surgeons, and 240 *dispensing* apothecaries; in all 1310, or one to 900 inhabitants: whereas the actual numbers are, 175 physicians, 1000 surgeons, 2000 *practising* apothecaries or *general practitioners*, and 300 *dispensing* apothecaries or chemists and druggists; in all 3475, or one to 345 inhabitants. Consequently, the expenses incidental to sickness are nearly three times as great in London as in Paris.

10. That the Royal College of Physicians in London were instituted for the purposes of "preventing improper persons from practising physic, punishing bad practice, and suppressing empiricism;" and that, to enable them to carry these objects into effect, they were vested with the powers of prosecuting, fining, imprisoning, and interdicting offenders.

11. That, in addition to the powers with which they were legally invested, they have usurped others, of which the following are some of the principal: 1. The re-examination of graduates in physic; 2. The limitation of the number of physicians, and consequently of medical students in the universities; 3. Their division into different ranks; 4. The establishment of a higher rank in favour of the graduates of certain universities, in which inferior medical instruction prevails; and, by an unparalleled inconsistency, the degradation to a lower rank of the graduates of other universities, in which medical instruction exists of a superior kind; and 5. By the prohibition, under a considerable penalty, to meet in consultation with doctors in physic who are not of their body, although they do not hesitate to attend with surgeons and apothecaries.

12. That the re-examination of graduates of universities, by a *private* College, whose rights as physicians are derived from their university diplomas; who are consequently no higher in rank than those whom they examine; who cannot even confer the title of Doctor in physic (in 1704, there were in their list *ten* licentiates who had no such title), and who have even a direct personal interest in the issue, is, besides being an usurpation, wholly preposterous.

13. That the division of admitted physicians into distinct ranks, which was at first entirely arbitrary, was in 1752 made to apply to particular universities, the fellowship being restricted to the graduates of Oxford and Cambridge, and the licentiates, notwithstanding the parity of rights expressly secured to the universities by the articles of union, taken principally from those of Scotland.

14. That, during the seventeenth century, the surgeons and apothecaries had become so numerous, the physicians remaining nearly stationary, that it became impossible to restrain them from practising physic; and that they subsequently, under a change of circumstances, and by virtue of particular charters, formerly acquired that privilege, whilst the College continued to prosecute *physicians*, not of their body, *for a similar exercise of their profession*.

15. That, in France and other countries, whilst the physician and surgeon observe some decency and reserve in their encroachments upon each other's provinces, the pharmacien or dispensing apothecary does not encroach upon that of either. In Britain alone is the *practising* apothecary, *surgeon*-apothecary, or *general-practitioner* known.

16. That he who exclusively devotes his attention to the practice of physic, must, capacity being equal, be the most skilled in that particular branch; and that he who depends for remuneration upon advice, has less powerful inducements to deviate from duty to his patients, than he who depends

for remuneration upon the quantity of drugs which he furnishes. Consequently, as in this country, nine-tenths of the practice of physic are in the hands of persons who depend for remuneration upon the quantity of drugs which they supply, and their interests being thus perpetually kept at variance with their duties, the organization of the different branches of the medical profession in foreign countries is far preferable to that which obtains in Britain.

17. That the effects, upon the interesrs of the public, of the mal-organization of the medical profession in Britain, are, to prevent their having a free choice of their physicians; to substitute surgical or pharmaceutical for medical aid of an appropriate kind; and greatly to enhance the expenses incidental to sickness.

18. That the effects upon the different branches of the medical profession, of the artificial, forced, discordant, disproportionate, and fluctuating state, into which they have been thrown by the causes mentioned, are injurious to all of them in a much higher degree than can readily be conceived.

19. That physicians who are not of the College in London, are precluded in England, when their professional character is libelled, from legal redress. Upon crossing the Tweed or the Channel, to prescribe for a patient, the eminent professors of Scotland and Ireland may be calumniated with impunity.

20. That, whilst in England, Members of the College of Physicians in London, of the Colleges of Surgeons in London, Dublin, and Edinburgh, and of the Society of Apothecaries in London, are, by the new Jury Act, exempted from serving on juries, there is no such exemption in favour of Doctors of physic, not of the London College, even the members of the Irish and Scotch Colleges of Physicians. They are also liable, in England, to be balloted for the militia, and to perform other obligations, from which the aforesaid persons are exempt.

21. That until the different branches of the medical profession be legally regulated a-new, upon unquestionable principles of public utility, it shall be a main object of the association of the faculty of physic in London, by means of a general confederation of the independent physicians, wherever they may reside, to afford support and protection to all doctors of physic, whether practising in town or elsewhere, who shall become members of their body, against the numerous civil degradations to which they are liable, as well as the usurpations of the College of Physicians in London.

22. That the public at large, the members of the legislature, and the ministers of the crown, are earnestly solicited to take this highly interesting subject into their early consideration, in order that efficient remedies may be applied to the very serious grievances complained of, and the disgrace wiped away of being, in respect to our laws in this department, centuries behind other civilized nations.

23. That petitions to the Legislature, and representations to the Ministers of the Crown, founded upon these resolutions, with such additions and alterations as may be judged necessary, be presented at the earliest practicable periods that may be found expedient, with a view to ulterior proceedings for the accomplishment of their object.

London, 12th of August, 1826.

TREATMENT OF FRACTURES.

To the Editor of THE LANCET.

SIR,—I beg the favour of you to insert the following observations in your next Number.

In THE LANCET, January 6th, there is a case of fracture of the leg, at Guy's Hospital, which terminated fatally; the treatment of which appears to me to have been very injudicious. In the first place, why put on splints in such a case, where, from the nature of the accident, much inflammation was to be dreaded? The application of leeches, too, only added to the irritation. If the limb had been laid in a pillow, placed on a flat piece of board, a *large* linseed meal poultice applied over the *whole* of the injured parts, and the pillow then slightly confined round by two or three bits of tape loosely tied, and this renewed once or twice in twenty-four hours, I have no doubt but the termination of the case would have been very different.

Nothing can be so contrary to the principles of good surgery, as to bind up recent fractures in splints. Most of the pain and inflammation which occur after fractures, are occasioned by confining the limb in bandages and splints. Suppose, for want of splints, the parts should become a little displaced? all may be easily put to rights when the inflammation is subdued. By this simple application of a linseed poultice, and *nothing else*, I have lately seen a very bad compound dislocation of the ancle, with fracture of the fibula, go on *favourably* from the *first*, without *pain*, *irritation*, or *fever*. I have, in the course of *many* years practice, *often* witnessed the *same* happy results from the *same* simple mode of treatment. I wish surgeons in Hospitals would lay aside their splints in the early management of fractured limbs, and give a fair trial to the plan here laid down.

COUNTRY SURGEON.

Jan. 9th, 18 7.

ROYAL INFIRMARY, EDINBURGH.

JAN. 27, 1827.

Case of Colica Pictonum, produced by the absorption of Lead used as dressing to an extensive Burn.

CULLEN has sanctioned the division of "cases" into the successful and unsuccessful. We have also another subdivision of reported surgery into records of failure from proper, and of fatality from improper, treatment. The classification might be extended further, by the establishment of another order, affording examples of the effects of good and bad practice in the same patient, and conducted by the same medical attendant. Thus, to poison a man in one stage of his illness, and subsequently to restore him, might be considered a familiar illustration of the latter description of remedial agency. Singular enough, all these varieties of pathological terminations have been esteemed almost equally instructive by the profession; but the cause is sufficiently obvious, for, the physician or surgeon who would merely know what to do in any given instance, would only be acquainted with half his duties, since knowing what to avoid is now thought just as important a part of his knowledge. At first sight, indeed, it might puzzle one to account for this electrical attribute of a "positive" and "negative" condition of the healing art, peculiar, perhaps, to this science alone, though the solution of the difficulty is quite evident, when it is considered that the excessive talent of some, the fertile ingenuity of others, the roguery of more, and the ignorance of as many as all the rest put together, have eventually discovered all that is good and all that is bad, and converted the exceptions of medicine into general rules. At all events, whatever may be the origin of this anomaly, and however burthensome its consequences must be to the memory of the practitioner, there can be no doubt of the necessity of being acquainted with the prohibitions, as well as the commands of therapeutics, as the case of Elizabeth Campbell will elucidate.

She was admitted on the 2d of November for an extensive burn of the abdomen, and contiguous parts of the body. The ulcer, in the usual time, assumed a healthy aspect, and continued to cicatrize in the usual manner, and nothing particular occurred in her case so far, except that her convalescence seemed more tedious than might be expected, even from so serious an injury, having never regained her wonted energy. Her pulse continued quick and feeble; her tongue slightly furred; her colour of a cadaverous hue; her features sharp, anxious, and attenuated; and, on asking her several times as to the cause of these appearances, she could assign no other reason for their presence than weakness and long confinement to bed. All her symptoms became more intense, and she now began to complain of a tendency to costiveness, and of weakness in the extremities, from her knees downwards. On the 16th of January, the abdomen became tender and tympanitic, and she complained of violent pain. An enema in the night brought away a large dejection, but she remained much in the same state, and was ordered a colocynth pill every sixth hour.—17th. The pain in the abdomen remained unabated; the bowels not opened during the night, and passed but about four ounces of urine. An injection of sulphat of soda and senna, and a dose of castor oil, were ordered to be taken immediately. From this to the 19th, she had taken at intervals castor oil, calomel, tincture of aloes, and had fomentations, and several enemata of turpentine administered, but without any effect. At eight o'clock on the night of the 20th, she was visited by Dr. Hunter, and ordered eight grains of scammony, and three of hyoscyamus every third hour, but, becoming worse during the night, an infusion of thirteen grains of tobacco was given in the form of injection, which freely evacuated the bowels in the course of about two hours. Since that period she remains in a state of extreme debility, and with a tendency to constipation.

During the treatment of the ulcer produced by the burn in this patient, an ointment containing an oxide of lead had been used, and the nature of her symptoms place it beyond a doubt that they were induced by the absorption of that substance, the extent and situation of the sore being both favourable to the introduction of the poison into the system. The temporising remedies employed in the acute stage of the disease, are not calculated to excite so much surprise, as that any preparation of lead should have been permitted in such a case at all. The wonder, indeed, is still increased, when it is recollected that almost every teacher of surgery warns his pupils against the use of this metal, as an application to extensive sores for any length of time. The complaint originating in this manner is consequently a very rare occurrence, and never, perhaps, to be seen in the wards of an hospital. Amongst the poor, however, the complaint sometimes arises in this way, with whom ground white lead is a favourite remedy for burns and scalds, many of which, every person who ever discharged the office of dresser in a public surgery, must have noticed coming in daubed over with this substance.

Having heard no allusion made to the cause of the colica pictonum in the present instance by Dr. Hunter, at his visits to the patient, the writer had the curiosity to consult the hospital journal, but he was much surprised at perceiving that silence was here observed also on that subject, by the Doctor's "fidus Achates." Now this omission is extremely ungenerous in Dr. Hunter; for the instances in which his practice affords instruction, being like angels' visits "few and far between," he might surely, on an occasion of this kind, make a virtue of necessity, suffer his heart to expand, and permit his pupils to profit even by his mistakes. The secret may probably have been reserved for official announcement in the clinical lecture room, where it will be heard as a novelty by the "select few;" while the majority of the pupils of the house, in consequence of this mercenary division of instruction, must remain in total ignorance of the most important feature in this case, and who might well address their superiors in the language of the exiled satirist of Rome:

"Sic libitum vano qui nos distinxit Othoni."

SCOTUS.

HOSPITAL REPORTS.

GUY'S HOSPITAL.

RUPTURE OF THE SPLEEN.

AN accident of this nature was admitted into the Hospital during the last admission-week of Mr. Bransby Cooper. The patient, a fine robust young man, fell from a building unto the ground, being a distance of about forty feet. He was brought into the Hospital at four o'clock in the afternoon, and lingered until eight o'clock on the following morning. The vital powers were in a state of extreme depression; but a few hours after admission there was a feeble attempt at reaction, and it was thought proper to open a vein in the arm—only a few drachms of blood, however, could be obtained.

On examination after death, there was found to be an extensive laceration of the spleen, with extravasation of blood into the abdomen. There were also four ribs fractured on the left side, a slight injury to the scalp, and a fracture of the left leg.

PARTIAL DISLOCATION OF THE ASTRAGALUS?

There is at this time in Cornelius Ward, a case on which some difference of opinion exists as to its nature. Sir Astley Cooper considers it to be a "well-marked instance" of a comparatively rare accident—partial dislocation of the astragalus. The patient is a young man, and was admitted under the care of Mr. Key, a few weeks since, in consequence of having received severe injury to the right leg. The accident occurred from the man having fallen from a height of six yards to the ground, and lighting on his feet. There was so much tumefaction immediately following the injury, that when the patient was admitted, and for some time after, it was certainly difficult to ascertain the precise nature of the injury done; crepitus was indistinctly felt at a short distance above the ancle, and in front of the joint was a very considerable hard swelling. In the course of a few days after the application of leeches, discutient lotions, and the observance of rest, the general swelling had so far subsided, as to admit of a more accurate examination being made; it was now apparent, on handling the limb, that the tibia was fractured at a short distance above the joint, and the fibula also broken, at about two inches higher up the limb. The malleolus externus, and malleolus internus, were found to maintain their relative situation; but in front of the ancle joint, there was a bony projection, equal in magnitude, and much resembling in figure, the fore part of the astragalus. Various and forcible attempts were made to reduce this swelling, but without effect; and the case was therefore simply treated in reference to the fractures. There is nothing material to remark on the progress of the case; the fractured bones became united in about the usual space of time, and at the period of making this report appear to be firmly consolidated. But the tumour on the instep remains, and hence a question arises as to its nature: it has really (jesting aside) proved a "bone of contention." We have before said, that the malleoli preserved their relative situations—such is the case now. The foot (if we except the swelling on the instep) is not deformed; there is the customary lateral motion; flexion and also extension, but more especially the former, is made with some difficulty.

Sir Astley Cooper's opinion of the case is, that the bone projecting at the instep is the astragalus, which bone, he says, is thrown forward, whilst, at the same time, the lower portion of the tibia has receded.

Mr. Key, on the other hand, considers that the swelling is occasioned by a fracture, with consequent displacement of the lower portion of the tibia, i. e. its articulatory portion, which has advanced, whilst, at the same time, the malleolus internus is left entire, and in its natural situation.

Mr. Key's reasons for doubting whether the tumour at the instep be the displaced astragalus, are these: that the lateral motion of the foot (which we know is performed between the astragalus and tibia) is seen in the present case to be performed between the projecting bone and the bones situated beneath, which he argues could not be the case, supposing the projecting bone really to be the astragalus; for in that case, the astragalus being thrown forwards on the os naviculare, would not move upon it, so as to give the extensive lateral motion capable of being produced in the present instance; neither would it admit of flexion and extension being made to the same extent. Added to these reasons against the probability of the accident being dislocation of the astragalus, Mr. Key remarks, that there is an absence of the deformity of the foot, characteristic of this accident.

We have endeavoured to give a fair and impartial statement of this case, with Sir Astley's *pros*, and Mr. Key's *cons*, and we confess ourselves really at a loss to know how to reconcile their "conflicting opinions." We suppose that nothing short of actual demonstration would be sufficient. Mr. Callaway considers it to be a case of dislocated astragalus; Mr. Morgan declines giving an opinion until the parts are *dissected*.

The case is well worthy of attention, and as the patient still remains in the Hospital, many of our readers may have an opportunity of seeing and deciding for themselves.

CASE OF RETENTION OF URINE FROM STRICTURE OF THE URETHRA, FOLLOWED BY ABSCESS AND SLOUGHING.—FATAL TERMINATION OF THE CASE.

R. L., a middle-aged man, of spare habit, was admitted into the Hospital on the 15th of November, under the care of Mr. Bransby Cooper, on account of retention of urine.

The patient, on his admission, stated that he had laboured under difficulty of passing his urine for several years, and that occasionally he had suffered with entire retention. He had passed but a very small quantity of urine for several days previous to admission, and not the slightest quantity during the preceding eighteen hours. He complained much of pain, and conceived great tenderness on making pressure over the pubic region; the bladder was evidently much distended.

Mr. B. Cooper attempted for a considerable time to introduce the catheter, but failing to do so, he directed the patient to be put in a warm bath, and to be bled from the arm to the amount of sixteen ounces. These measures being complied with, faintness was produced, and the catheter was again attempted, but without success. A short time after his removal from the warm bath, the urine began to flow, or rather to ooze away, but to such an extent that the bed-clothes were observed to be very wet, and the pain, with the distention of the bladder, were materially lessened.

Mr. Cooper directed that warm fomentations should be applied over the pubic region; and prescribed a dose of aperient medicine, with two grains of calomel, and one grain of opium, to be taken at bed time. The patient was admitted at noon.

16. The urine has continued to dribble away at intervals throughout the night in the same manner as yesterday; the bladder is still much distended, and there is great tenderness on pressure. The patient has occasional hiccup; the pulse is small, but not quick; the tongue slightly furred, and with numerous fissures apparent on its surface.

Mr. Cooper made efforts for a great length of time to pass the catheter, but was prevented, as he stated, by a very firm stricture, situated in that part of the urethra immediately under the arch of the pubes. Mr. Cooper considered that the symptoms were not sufficiently urgent to warrant an operation; he therefore directed twelve leeches to be applied over the pubes, and the warm fomentations to be continued. In the evening the patient was suffering very much, and was visited by Mr. Callaway, who succeeded in introducing a catheter, and drew off nearly four pints of high-coloured urine. This afforded much relief to the patient. The catheter was retained in the bladder by means of tapes; a further application of leeches was directed, and a dose of calomel and opium.

17. The patient is much better; the catheter now remains in the bladder unplugged, the urine being allowed to flow through it. Ordered a dose of castor oil.

19. The catheter removed to-day, and one of larger size introduced without much difficulty.

20. On visiting the patient this afternoon, he complained of having had two fits of shivering in the night, and he obtained but little rest. The pulse is quick, the skin hot and dry; the bowels have not been moved since yesterday. He does not complain of pain in the bladder, or at the seat of stricture, and the urine passes freely through the catheter, which is still retained in the bladder. Ordered to take two grains of calomel and one grain of opium immediately, and to repeat the dose at bed time; an ounce of castor oil to be given in the morning.

21. The febrile symptoms have in a great measure subsided; the bowels have been

freely acted upon. The catheter came out this morning, and was introduced again with some difficulty,

22—23. Much the same; the catheter retained in the bladder.

25. The catheter came out in the course of the night, and has not again been introduced. The patient has passed urine three times without the instrument, and says that there is no obstruction.

26. We learn from the dresser that yester-evening the patient became much worse; he was irritable and restless; the pulse quick and sharp, the tongue red and dry, and the skin hot. The catheter was introduced by Mr. Callaway, but was not continued in the bladder.

This morning the pulse is 85, and sharp in its beat; the bowels have been moved several times, and the tongue is moist.

The catheter was introduced this morning, and is now retained in the bladder; the urine passes freely through, but the patient complains of much pain in the urethra, about its membranous portion.

Ordered to have ten leeches applied over the part in pain, and to take

Calomel, one grain;
Opium, half a grain;

every six hours, with the following draught:

Solution of acetate of ammonia, half an ounce;
Antimonial wine, half a drachm;
Camphor julep, one ounce. Mix.

27. On removing the catheter this morning, some pus was observed to flow from the urethra. Mr. Cooper supposes that an abscess has formed, and burst into the urethra. The pulse is quick and sharp, the skin hot and dry, the tongue is also dry, and the patient complains of thirst.

Ordered to continue taking the calomel and opium; the catheter not to be introduced, if the urine flows freely.

28. There is a soft fluctuating swelling of about the size of a walnut, immediately anterior to the scrotum; this tumour disappears on making pressure, from its contents passing into the urethra. There is swelling and tenderness in the perineum; the patient complains of the urine scalding when it passes. The pulse is 95, and of that peculiar character which denotes great irritation existing in the system; the tongue is dry, and morbidly clean; the bowels moderately open.

Ordered to continue the calomel and opium, and to take two grains of the sulphate of quinine from a wine-glassful of the infusion of roses, three times a-day.

The only local treatment adopted, consisted in *making pressure on the swelling by means of strapping and a roller, in order to prevent matter from accumulating!*

29. The swelling in front of the scrotum has increased; it may still be emptied into the urethra by pressure. The urine now is almost constantly dribbling away, and a bladder is now worn tied to the penis. *No further local treatment!*

Ordered to take twelve minims of the black drop at bed time, and to continue the other medicines.

30. Much the same, both as regards local and constitutional symptoms.

December 2. We find the patient this morning very low and dejected: he says that he is about to die. The pulse is 100, and small; the tongue dry and red, and the appetite bad. The abscess, situated anterior to the scrotum, frequently becomes painfully distended with urine, when the patient minges; but it is got rid of by making pressure, and this he does as often as it becomes filled. Mr. Cooper directed four ounces of port wine to be given in the space of twenty-four hours, with arrow root; to continue the sulphate of quinine, and anodyne draught at bed time. *Local treatment left to the vis-medicatrix naturæ!*

3. The patient obtained a better night; otherwise, he is much the same as yesterday.

4. The abscess has greatly enlarged; it is harder to the touch; and the integuments to some extent around, have a dark red colour; whilst, in the centre of the swelling, there is a gangrenous spot nearly of the size of a shilling. The constitutional symptoms are in unison with this state of parts; the pulse is small, the tongue dry and red, and the patient has occasional hiccup.

An opening was made in the slough, and a considerable quantity of ill-conditioned pus, mixed with urine, made its escape. Ordered to apply a port wine poultice to the part, and to continue taking the quinine, &c. The quantity of wine increased to eight ounces daily.

5. The left nates is swollen, and the integuments are of a dark red colour; Mr. Callaway, who saw the patient this morning, attributes the circumstance to infiltration of urine in the cellular membrane. There is no further increase of sloughing in the integuments; a portion of the dead part is loosened, and thus an opening is formed leading into the urethra, through which a considerable quantity of urine flows. A large linseed-meal poultice was ordered to be applied to the nates, the port-wine poultice to be continued to the abscess, and in front of the scrotum. The pulse continues feeble, and the hiccup is occasional; the patient relishes his port-wine and arrow root.

6. There are numerous small vesications to be observed on the under part of the penis. The nates on the left side presents the same appearance as yesterday; several deep incisions were made this morning, but

no fluid escaped. A crucial incision was made in the slough, over the abscess.

The pulse continues thready and quick; the tongue is red and dry.

Ordered to take two grains of the sulphate of quinine, with half a grain of opium to night, and the like dose to morrow morning. Eight grains of the carbonate of ammonia, with a drachm of tincture of cardamoms and a wine-glassful of peppermint water, to be taken every six hours. Port wine poultices to the part.

7. The patient to all appearance is moribund; the pulse is scarcely to be felt, and the hiccup is distressing; efforts are made to vomit, but little is rejected from the stomach. The appearance of the parts is much the same as yesterday.

On visiting the Hospital on the morning of the 8th, we learned that the poor man died on the preceding evening about eight o'clock.

A post-mortem examination took place, and upon inspecting the urethra, there was found to be a stricture of considerable firmness at the posterior part of the bulbous portion of the urethra. An abscess had formed, externally to the urethra, which was partly anterior to the stricture, but also extended some way below the membranous portion of the urethra. There was a false passage in the urethra, large enough to admit of the point of a catheter, and it was of about two inches in length; this false passage commenced anteriorly to the stricture, and terminated at a short distance behind it, so that in all probability the catheter had taken this course behind, or rather beneath, the natural channel, and had thus as it were evaded the stricture. The nates were found to be in a sloughing state, to some depth; there was not, however, any evidence of this having arisen from the infiltration of urine.

OPERATIONS.

The operations performed at this Hospital since our last report, are—*amputation of the leg*, by Mr. Key, in consequence of an extensive and inveterate ulcer; *excision of a cancerous tumour* from the lip, and the removal of a diseased sequestrum of bone in a case of necrosis:—both operations by Mr. Key.

Lithotomy. On Tuesday, January 16th, Mr. Key performed the operation of lithotomy on a boy of twelve years of age. The only difficulty which occurred was in the extraction of the calculus, and this arose from its peculiar shape: it was of an oblong figure, and, in grasping it with the forceps, was placed with its long axis across the wound; it therefore became necessary to turn the stone, and this being effected, the extraction was easily made. The patient is doing well.

ST. THOMAS'S HOSPITAL.

The cases admitted into this Hospital of late, have been for the most part of an uninteresting description. From Mr. Tyrrell's patients admitted on the 21st of December, we select the following cases as worthy of a brief notice:—

George's Ward, No. 11.—J. P., ætat. 34. Mr. Tyrrell entitles this case, "*Irregular hard tumours, connected with the fibrous texture of the fore arm and carpus.*"

On looking at the right arm, it is evidently very much swollen; and, on examining it with the hand, we find numerous hard and irregular tumours beneath the skin, varying in size, and apparently situated in the cellular membrane beneath the integuments, but, at the same time, attached to the fascia of the fore arm, being immoveable at their base. These swellings are principally situated on the back part of the fore arm, but the disease also extended to the wrist; the tumours are not painful, nor does the patient evince tenderness on pressure.

The patient's general health is good; he states that he had a venereal sore on the penis nine years since, and also one about three years ago, with swelling of the inguinal glands. He took turpette mineral (as he terms it) for the cure of these diseases, but was not salivated.

Treatment.—Mr. Tyrrell directed a blister to be applied to the arm, and prescribed:—

Oxymuriate of mercury, one-eighth of a grain;
Decoction of sarsaparilla, 2 ounces;
to be taken three times a-day.

On visiting this patient, on the 20th of January, for the purpose of making a final report, we find that the treatment employed has been highly successful. The limb is reduced to half its former size, and there is now only very slight thickening to be traced here and there beneath the skin. Three blisters have been successively applied to the arm, and the alterative medicine has been continued.

NODES OCCURRING WITHOUT THE PREVIOUS USE OF MERCURY, AND INDEPENDENTLY OF SYPHILIS.

The patient in this case is a young man of florid complexion, and was admitted by Mr. Tyrrell on the 21st of December, on account of a swelling situated on the upper and back part of the ulna, and also one upon the fore part of the left tibia, which Mr. Tyrrell denominated nodes. If the patient's statement can be at all relied upon, this case affords an instance of nodes occurring independently of a syphilitic taint of constitution, or of the previous use of mercury. He asserts that he has never laboured under

any form of venereal disease, nor has ever taken a particle of mercury—at least, not to his knowledge; and, decidedly, not so as to affect his mouth. He has been, throughout his lifetime, subject to rheumatism; and about three months before admission, was confined to his bed with an attack of acute rheumatism, to which succeeded these swellings on the bones. The pain in the swellings is greatest at night, and is increased by warmth.

Under the repeated application of blisters, with the exhibition of mild alteratives, combined with sarsaparilla, the exostoses are gradually lessening.

EFFICACY OF HYDROCYANIC ACID IN DERANGEMENTS OF THE STOMACH.

We believe that the powers of hydrocyanic acid in pectoral diseases have been much overrated, and, as a consequence thereof, it has fallen into disrepute, and indeed we may say almost into disuse. But the hydrocyanic acid is undoubtedly a valuable remedy in many affections of the stomach; for a knowledge of this fact, and the introduction of this medicine into practice in such cases, the profession is indebted to Dr. Elliotsion, who, as he says, discovered by a mere accident, in fact, by a mistake, its extraordinary efficacy in derangements of the stomach. We have selected the two following cases, at present in the Hospital; the one as illustrative of the remedial powers of hydrocyanic acid in the affection of the stomach called water-brash, and the other as showing its efficacy in checking sickness, dependant on excess of irritability in the stomach.

Case 1.—A young woman admitted into Ann's Ward under the care of Mr. Tyrrell, on account of extensive ulceration of the face, which Mr. Tyrrell denominates—*noli me tangere*. For upwards of a year and a half the patient has been subject to occasional attacks of pain at the pit of the stomach; and during these attacks, a quantity of clear fluid rises into the mouth, constituting what is usually denominated pyrosis or water-brash. Mr. Tyrrell prescribed various medicines without benefit. On the 9th of December, Dr. Elliotson saw the patient, and prescribed two drops of the hydrocyanic acid to be taken three times a-day. On the 12th, the amendment in the disease was but slight, and the dose was increased to three minims. On the 30th, the pain was rather more frequent, and Dr. Elliotson further increased the dose to four minims three times a-day. On the 6th of January, six minims were directed to be taken three times a-day. On the 13th, the report made on the case was, that no fluid had risen into the mouth during the week, nor had any pain been felt. On the 20th, the patient was reported perfectly free from any stomachic affection.

Case 2.—This patient, a middle aged man, was admitted into Abraham's Ward under the care of Mr. Tyrrell, on account of simple oblique fracture of the right humerus, near to its centre. About three weeks after admission, the man became affected with constant sickness on taking food; he did not complain of any particular pain at the pit of the stomach; he had no febrile excitement, nor did the appearance of the tongue denote any affection of the mucous membrane of the stomach. It appeared to be simple excessive irritability of the stomach, dependent on some unknown condition of the organ. After various means made use of by Mr. Tyrrell had failed in alleviating the distressing symptom, Dr. Elliotson was requested to see the patient, and he prescribed two minims of the hydrocyanic acid to be taken three times a-day. A few days afterwards, the dose was increased to three minims; the good effects of the medicine were soon rendered sufficiently obvious; the sickness was checked after a few doses had been taken; and, at the end of the week, the patient was reported as free from any gastric affection; the medicine was then discontinued.

There have been no operations performed at this Hospital for several weeks past. This was remarked to Mr. Joseph Henry Green; who replied that they (the surgeons of St. Thomas's) cured their cases, and THUS PREVENTED THE NECESSITY OF OPERATIONS.—Lord bless us!

RUPTURE OF THE INTESTINES AND MESENTERY, AND LACERATION OF THE KIDNEY; OCCASIONED BY A LOADED WAGON PASSING OVER THE ABDOMEN.

A poor boy, fifteen years of age, was brought to the Hospital about nine o'clock on Saturday morning, January 20, having received severe injury to the abdomen. He was placed in Edward's Ward, under the care of Mr. Travers. The accident occurred under the following circumstances:—The boy was riding on the shaft-horse of a team employed in drawing a wagon laden with coals, when he unfortunately fell, and not being able to extricate himself, the wheel of the wagon passed directly over his body. He was conveyed to the Hospital about half an hour after the receipt of the injury. We understand that when he was first admitted, he was quite sensible. The most prominent features of the case were, constant pain in the abdomen, and the low state of the vital powers,—the heart's beat being scarcely perceptible. It was about noon when Mr. Travers saw the patient, at which period also we first visited him. The following

symptoms were then present: the face had the paleness of death, the lips were exsanguineous, and the whole surface of the body was cold; there was no perceptible pulsation in the radial artery, and that of the carotids was very feeble, whilst the heart was rather fluttering than beating. There seemed to be a disposition to sleep; but when spoken to, the poor boy readily replied to the questions put by Mr. Travers; he was quite sensible, and referred his pain to the abdomen, which was tense, and tender on pressure. He had taken frequently of brandy and water from the time of admission, but the stomach had uniformly rejected it; there was no vomiting at other periods. When Mr. Travers was examining the boy, he laid on his back; but soon as he had completed his inquiries, the boy *threw* himself on his left side; we say *threw*, because the movement was instantaneous, and, we may say, almost of a convulsive nature. There was something exceedingly characteristic in this apparently trivial circumstance.*

Previous to the visit of Mr. Travers, the dresser had very properly introduced a catheter, and drawn off a small quantity of bloody urine; but as the poor boy was complaining of a desire, with inability, to pass his urine, the catheter was again introduced. A few ounces only of bloody urine were drawn off; Mr. Travers imputed the symptom of a desire to make water, to hæmorrhage into the bladder, which he thought was going on.

The only further means employed in this case, were the occasional exhibition of brandy, and the application of warmth to the feet. The poor boy lingered until the evening, when he expired.

The body was examined on the following morning (Sunday). We were not present at the inspection, but learn from the dresser that the ileum was found to be ruptured in three places; the mesentery was torn, the right kidney was lacerated, as was also the emulgent vein. There was a considerable effusion of blood into the abdomen, but no perceptible feculent matter; an intestinal worm was, however, found floating in the extravasated fluid.

[Mr. Travers has presented the morbid parts to Mr. J. H. Green for his *private* museum, commonly called the museum of St. Thomas's Hospital. We are glad to find that the physicians of this Hospital, having been denied the use of the preparations in *Mr. Green's museum*, have determined on forming a collection, which is to be the property of the Hospital. We observe Dr. Elliotson particularly active on this occasion.]

* Mr. Travers termed it a "*saltatory movement.*"

ST. BARTHOLOMEW'S HOSPITAL.

Two operations were performed this week by Mr. Lawrence; one, the removal of an oblong fatty tumour, nearly as large as the pancreas, and somewhat of its shape, situated between the trapezius and inferior portion of the sterno-cleido-mastoideus muscles. Mr. Lawrence, supposing part of the tumour to be deeply seated, made the incision wider than its ultimate size warranted.

The other was the removal of an extraneous cartilage from the knee joint; it had been of two years formation, and caused but little pain; when it was between the articular surfaces it threw the patient down. Mr. Lawrence having secured the cartilage between the finger and thumb, and cut down upon it, it leaped out, about the size of a French bean, smooth, and of a white colour.

A man, æt. 43, was admitted into Luke's Ward, with disease of the testicle. At the time of admission, the testicle was considerably enlarged (as large as the head of a child of eighteen months or two year's old,) of a circular form, and of various degrees of firmness, in some parts hard and unyielding, and in others soft and fluctuating, the opposite testicle being of its natural form and situation. The patient was florid, and in a state of good general health; he had no other affection at that time. He stated, that seven years ago he had received a severe blow on the affected part, and that from this occurrence the testicle had gradually enlarged to the present period, when it had acquired the size above described, and during that period of seven years he had been thrice tapped. The patient was put upon a strict antiphlogistic regimen, and various local antiphlogistic means were resorted to, and persevered in for more than three months without success, either as to diminution in the size or firmness of the tumour. Mr. Vincent then determined on extirpation, the other surgeons having been consulted, and having decided in favour of it. The health of the patient had not suffered since his admission. The extirpated part was afterwards examined by the surgeons. On cutting through the tunica vaginalis, that cavity seemed divided into two parts by adhesion about its middle. Each cavity contained a small quantity of straw-coloured fluid. On making a section of the testicle, its substance had every appearance of scirrhus, being hard and inelastic, and divided into cells by numerous fibrous bands extending in every direction through it. In one of these cells was found a soft brain-

like matter, with streaks of blood much resembling fungus hematodes; in a few places were found flakes of caseous or scirrhous matter.* The other surgeons considered it to be of a malignant nature, and their opinion was strengthened by the brain-like appearance of the substance in the cell.

After the removal of the testicle, the patient experienced symptoms resembling those of enteritis, for which he was twice bled, together with other appropriate means. Seven days after the operation he died, without any apparent urgent cause.

Post Mortem Examination.

The incision made into the scrotum had suppurated freely, without the slightest adhesion having taken place. The inguinal canal contained some healthy pus; there was nothing remarkable with respect to the chord or internal ring.

On opening the abdomen, the muscles and fat had a most healthy appearance, as had also the viscera at first sight; but upon withdrawing the small intestines from the pelvis, a portion of about six inches was found highly vascular and red, and the internal surface ulcerated. Behind the transverse arch of the colon, below the transverse portion of the duodenum, and below the pancreas on the inner side, and attached to the pelvis of the left kidney by its adipose tissue, was found a tumour (as large, or even larger than the diseased testis) covered by peritoneum, cellular, filled with medullary matter, similar but thinner than that contained in the cells of the testicle above mentioned; there were two or three small osseous specks in the aorta, and the inner membrane of the vena cava was very red; the renal capsule retained its ordinary appearance, so did the mesenteric glands; the kidney was smaller and lighter than usual; there was slight adhesion between the convex surface of the liver and diaphragm; the vesiculæ seminales were much enlarged, and filled with a dark brown fluid; the left vesicula was lighter coloured than the right.

PARALYSIS NEARLY COMPLETE, OF THE LOWER EXTREMITIES, FROM A FALL ON THE FEET, CURED BY APPLICATION OF MOXAS.

A man, aged 30, was received into Rahere Ward under Mr. Lawrence. Seventeen days before his admission, being frightened, he jumped out of a window about twelve feet high; he states that he alighted on his feet, and subsequently fell forward; he was unable to move, and, consequently, was carried to bed. He remained insensible the first few hours, during which time he was bled; upon becoming sensible, (after the bleeding,) he recollected having great pain in his back and legs; he had not the slightest power over the lower extremities, and there was partial loss of sensation; the urine and feces were discharged voluntarily, and without difficulty; the pain during the first week was very severe, particularly when he coughed or sneezed, and extended from the back down the thighs to the feet; after this time it gradually subsided; and now, when lying quietly in bed, is perfectly easy; but on turning, or when pressure is made on the left side of the lumbar vertebræ, he complains of a dull, though not very severe pain. The sensation is nearly restored, but there is a sense of numbness remaining. He cannot raise or draw up his legs in the slightest degree. When some time had elapsed without any restoration of power in the limbs, the lower part of the spine was carefully examined, the patient being supported in the sitting position. A little bulging was observed in the lumbar region; some pain was experienced on pressing the part, and the patient was sensible of weakness in the same situation. A moxa was applied on each side of the spine, and the application was followed by considerable return of power in the limbs, as they still remained feeble; two other moxas were placed lower down, and a further great improvement was derived from them. As he was able to walk very well with a stick, he was discharged on the 13th of December.

ULCERATED CARCINOMA OF THE RIGHT BREAST.

This was a hopeless case of cancer, in a short thin woman, ætat. 64, of dark complexion. The right mammary gland was a hard incompressible tumour fixed to the pectoral muscle, and with a large ulceration on the surface. There were two smaller swellings of similar hardness: one in the axilla, the other above the clavicle. The original disease had begun some years ago; it ulcerated and became painful in the last twelve months. Palliation was all that could be attempted in this case; the liquor opii. sedativus was employed externally, and opium given largely, but without much relief.

On examination after death, the original tumour presented the usual very firm semi-cartilaginous scirrhous structure, with opaque yellowish streaks interspersed through it. The axillary tumour, and that above the clavicle were composed of lymphatic glands; exhibiting the same morbid structure as the breast. Other lympha-

tic glands behind the first bone of the sternum were in like manner diseased. A few small tubercles were found in the lungs, chiefly on their surface. The liver was studded, both in its substance and on the surface, with white and tolerably firm tubercles of various sizes. In other respects it was healthy. Two small tubercles were found on the external surface of the mucous membrane of the stomach, and a similar one in the corresponding situation of the small intestine. The sternum was sawed through, and two parts were observed very different in colour from the rest of the bone, but no change of texture could be detected.

SLOUGHING ULCERATION PRODUCED BY CONTAGION.

Mary Himley, aged 43, was admitted under Mr. Lawrence, September 1. A very painful inflammation and induration of the integuments took place on the middle of the calf of the leg, a month before she came to the Hospital, and slowly ulcerated. She was in good health, had never been similarly affected, nor had sores of any kind. When she came to the Hospital, the sore was inflamed from much walking. Rest, leeches, and aperient medicines, with milk diet, soon reduced the inflammation, and brought the sore into a healing state. Cicatrization was considerably advanced when inflammation took place in the upper margin of the sore, and a small sinus was discovered and divided; its surface had a dark colour; this unfavourable appearance increased until it spread into a foul sloughy sore, as large as a half crown-piece, with jagged edges and deep red margin of considerable extent; this unfavourable change in the sore did not begin until two patients with bad sores had come into the two adjoining beds; one with phagedenic ulcer of the foot, and the other an old woman with extensive mortification of the leg, accompanied with the most fetid discharge. As the sore of the patient Himley continued to spread, with a dark sloughy surface, great surrounding inflammation, and severe pain, she was removed to another ward.

November 9. Nothing was done but poulticing the sore, that the effects of the change of situation might be more accurately estimated; the pain was immediately relieved, the surrounding inflammation soon declined, and the aspect of the sore mended. As at the end of two days its surface was not rendered clear, cinnabar fumigation was used for about a week; it speedily removed all remains of sloughy character, and the ulcer healed rapidly.

TO CORRESPONDENTS.

Well might X.—R. L. E.—Z., and a host of others, ask for some explanation of the case of *Strangulated Hernia*, reported at page 495 of our 176th Number. It was our intention to have left it as another *trap* for Jemmy Johnson, but its peculiarities demand an early notice.

The "Experiments and Remarks on the Circulation of the Blood" by Mr. Davies, of *Hertford*, shall be inserted, if possible, in our next Number.

The announcement required by D. C. is an *advertisement*.

We beg to assure "*Amicus*" that no error or neglect has occurred relative to our Wrappers—the current number has *invariably its proper cover*; but the *back numbers* are stitched up with *any covers* that we happen to have by us, if their *proper covers* are out of print.

We are surprised that "A Friend to Reform" should persist in his hypothesis. If he can prove the words we formerly quoted to be "a nullity," we will yield the point. We ask him, if the words are "merely a form," why were they omitted in the clause relative to the rights of chemists and druggists?

"A....t" states that Mr. C. Bell has resigned the Lectures in Windmill-street. What! so soon after the appearance of his name in the advertisements? Impossible! We shall, however, look into this matter.

We thank an "Undergraduate of Cambridge" for the work on Cancer, but it appears to be a most wretched concern.

"E. G." is informed, that the poor devils never had a "Bequest."

The following Works *have been received.*

L' Agent immediat du Mouvement Vital, devoilé dans sa nature, et dans son mode d'action chez les végétaux, et chez les animaux. Par M. H. Dutrochet, Correspondant de l'Institut, dans l'Academie Royale des Sciences, &c.

The author of the above work endeavours to elucidate the vital movement in the animal and vegetable kingdoms, by an explana-

tion of the course that the juice takes in plants, and the causes of its propagation. M. Dutrochet thinks that the progression of liquids in vegetables, and in living bodies generally, is the result of a particular form, capable of accumulating them in a much greater quantity than could be done by simple capillary action. He establishes his opinion upon a series of experiments, in which, having placed in water a sac formed of an organic membrane, filled with very thick animal paste, he saw, after half an hours immersion, the solid paste in great part driven from the sac by the water, which traversing its parietes, introduced itself into the cavity of the sac. Different experiments led the author to recognise that every time an organic membrane is interposed between two liquids of an equal density, a movement of one of those liquids takes place across the membrane; the movement being in general effected from the liquid possessing the least towards that possessing the greatest density. The power by which the liquid is pushed into the interior cavities, M. Dutrochet calls *endosmose*, and the contrary action, *exosmose*.

The experiments of the author also show that alkalies placed in the interior of a membranous sac, always produce endosmose; while the acid under the same circumstances, produce exosmose, whatever may be the density of the liquids. This last circumstance leads the author to believe that the cause of such a phenomenon is purely electrical, and that the membrane in which it operates, holds the place of a permeable Leyden jar. In modifying his experiments according to this idea, M. Dutrochet placed an organic membrane in the inferior part of the tube, between two heterogeneous liquids, the one within the tube, the other without; by these means he succeeded in making the lowermost liquid pass into the tube, and in elevating it so as to make it pour over the sides. This effect is always maintained, up to the putrefaction of the membrane; all animal membranes, all vegetable textures have afforded, the same result. The author also remarked, that in making the two extremities of a galvanic pile communicate with each side of the membrane, the liquid is always carried from the positive pole, or the least dense, towards the negative pole, or the most dense. The elevation of the temperature sensibly favours the endosmose, on which account the author thinks that inflammation in animals may be considered as a perendosmose. Whether the theory be correct or not, it is not our intention at present to discuss; we have seldom perused a work possessing so much valuable physiological knowledge, and we strongly recommend it to the attention of our readers.

Pharmacopée Française, ou Codex de Medicamens, nouvelle Traduction du Codex Medicamentarius, sive Pharmacopœa Gallica. Par D. F. S. Ratier, et M. O. Henry, fils, &c.

This work is an able translation of the French pharmacopœa, and in addition, contains a synoptical table of all the mineral waters of France, with an account of their chemical properties, and the names of the chemists by whom the analyses have been made. This part of the work will be found interesting to most English readers.

The unnoticed Theories of Servetus; a Dissertation addressed to the Medical Society of Stockholm, by G. Sigmond, M.D., late of Jesus College, Cambridge, and formerly President of the Royal Physical Society of Edinburgh. London, 1826. I. H. Burn, pp. 72. 8vo.

Myology, illustrated by Plates on a peculiar construction, in Four Parts.—Part IV. containing Muscles on the Face and Eye, Anterior and Posterior Parts of the Neck, with the Muscles of the Perineum, completing the whole of the Muscles of the Human Body. By E. W. Tuson, Lecturer on Anatomy and Physiology, Member of the Royal College of Surgeons of London, and late House Surgeon to the Middlesex Hospital. London, 1826. Callow and Wilson. Folio.

Treatise explanatory of a Method whereby Occult Cancer may be cured, together with Observations and Practical Directions for its Treatment in the Ulcerative Stage. By W. Farr, Surgeon, M.R.C.S. in London, and late Surgeon to the Hospital in the Island of Anholt, Author of a Treatise on Scrofula, &c. Second Edition. London. G. Wightman, 1825. pp. 79. 8vo.

A Syllabus of Surgical Lectures on the Nature and Treatment of Fractures, Diseases of the Joints, and deformities of the Limbs and Spine, containing descriptions of the modes of applying Twelve New Apparatuses, illustrated by Twelve Plates, with Cases showing the advantages arising from the plans of treatment recommended by I. Amesbury, F.S.A. F.L.M.S., Surgeon to the South London Dispensary, &c. London. Underwoods. 1827. pp. 140. 8vo.

Crook's Manual of the Phrenological Organs. London. S. Leigh, 1827. 18mo. Folding Card.